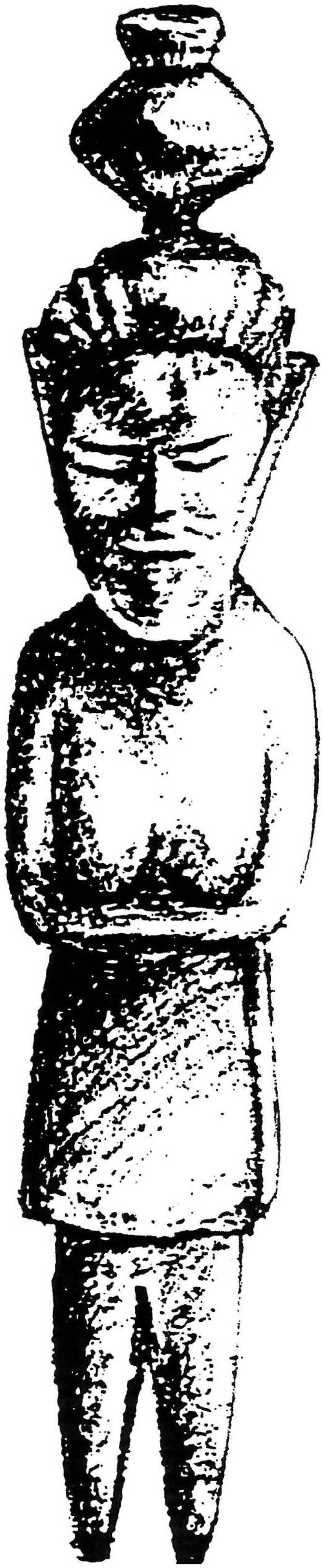
MOTHER TONGUE

JOURNAL OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY

Issue XVIII • 2013 • Commemorating the **50th Anniversary of J.H. Greenberg's *The Languages of Africa*** (1963)



CONTENTS

1 **Obituaries:** Christy G. Turner; Peter A. Michalove

Articles: Joseph H. Greenberg and His Legacy

9 Move along, there's nothing to see here: How the SLP ban was pronounced *Pierre J. Bancel*

17 The Niger-Congo Hypothesis and Its Perspectives in the 21st Century *Kirill Babaev*

23 Joseph Harold Greenberg: A Tribute and an Appraisal *Harold C. Fleming*

43 Why Is Africa So Linguistically Undiverse? Exploring Substrates and Isolates *Roger Blench*

79 Proto-Nostratic Morphology *Allan R. Bomhard*

143 The Early Dispersions of *Homo sapiens sapiens* and proto-Human from Africa *Harold C. Fleming / Stephen A. Zegura / James B. Harrod / John D. Bengtson / Shomarka O.Y. Keita*.

189 On the Origin of Milyan Verbs *Vitaly Shevoroshkin*

211 Global Etymologies and Alfredo Trombetti *Shamil Nafiqoff*

219 Notes on the Moscow Conference on Long-Range Comparison *Peter A. Michalove†*

Book Review

221 *Desi Words Speak of the Past: Indo-Aryans in the Ancient Near East,* by Liny Srinivasan Reviewed by *John D. Bengtson*

Notices & Miscellanea

MOTHER TONGUE **Issue XVIII, 2013**

Journal of the Association for the Study of Language in Prehistory

---

50th Anniversary of J.H. Greenberg's *The Languages of Africa* (1963)

OFFICERS OF ASLIP

President:
Michael Witzel witzel@fas.harvard.edu
Department of Sanskrit Tel. 617-495-3295
& Indian Studies
Harvard University
1 Bow Street
Cambridge, MA 02138
U.S.A.
http://www.people.fas.harvard.edu/~witzel/mwpage.htm

Vice-President:
John D. Bengtson palaeojdb@hotmail.com
5108 Credit River Drive Tel. 612-839-3649
Savage, MN 55378
U.S.A.
http://jdbengt.net/

Secretary-Treasurer:
Michael T. Lewis lewismtc@rcn.com
20 Duane Avenue Tel. 617-964-0978
West Newton, MA 02465
U.S.A.

Information Officer:
Jonathan Sherman Morris jonathanmorris1964@gmail.com
São Paulo, Brazil Tel: 5511-31512667

MOTHER TONGUE **Editor:** John D. Bengtson (see above)

Technical Advisors:
Brita M. Bengtson http://www.bmgb.net/index2.html
John Robert Gardner atman@vedavid.org

BOARD OF DIRECTORS

Václav Blažek (Masaryk University)
Ronald Christensen (Lincoln, Mass.)
Murray Denofsky (Somerville, Mass.)
Frederick Gamst (Los Osos, Calif.)
Phillip Lieberman (Brown University)
Jan Vansina (Madison, Wis.)
Allan R. Bomhard (Charleston, S.C.)
Nicholas Davidson (Center for Avestic Research)
Harold C. Fleming (Boston University)
John Robert Gardner (Gloucester, Mass.)
Michael Puett (Harvard University)
Stephen L. Zegura (University of Arizona)

COUNCIL OF FELLOWS

Raimo Anttila (University of California, L.A.)
Luigi Luca Cavalli-Sforza (Stanford University)
Vyacheslav V. Ivanov (Russ. Academy of Sci.)
Colin Renfrew (Cambridge University)
Vitaly Shevoroshkin (University of Michigan)
Ofer Bar-Yosef (Peabody Museum, Harvard)
Vladimir A. Dybo (Russian Academy of Sci.)
Sydney M. Lamb (Rice University)
Merritt Ruhlen (Stanford University)
George Starostin (Russ. State Univ. of Humanities)

ISSN 1087-0326

ASLIP Homepage: http://aslip.org

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

Introduction to Mother Tongue XVIII (2013)

The publication of Joseph H. Greenberg's *The Languages of Africa* (1963), whose fiftieth anniversary we commemorate in this issue, was one of the monumental events in genetic linguistics of the twentieth century, not only for its authoritative classification of African languages, but for genetic classification of languages in general.

This book was actually the culmination of a series of articles begun in the late 1940s (Greenberg 1948, 1949-50, 1954a, 1955), but the 1963 book is the one usually cited as containing the definitive statement of classification.

In the first chapter of this book Greenberg laid out the three "fundamentals of method" used in his classification of African languages, and which he followed in his subsequent classifications of Papuan and Sundaland languages (Indo-Pacific: 1971), languages of the Americas (Eskimo-Aleut, Na-Dene, Amerind: 1987), and languages of Eurasia (Eurasiatic: 2000, 2002). These three principles were: (**A**) the sole relevance of resemblances involving both sound and meaning, (**B**) mass comparison as opposed to isolated (binary) comparison, and (**C**) only linguistic evidence is relevant to genetic classification.

A. **The sole relevance of resemblances involving *both* sound and meaning:** This principle was essential, for example, in disentangling the so-called "Nilo-Hamitic" languages and "Hottentot" (now preferably Nama) from Afroasiatic ("Hamito-Semitic"). In the first case Carl Meinhof postulated a "Nilo-Hamitic" taxon based on some lexical parallels (now considered loans mainly from Cushitic), but more importantly on the fact that these languages have grammatical gender. Greenberg pointed out that the "Nilo-Hamitic" gender morphemes (*l*-masculine / *n*-feminine) bore no *phonetic* resemblance to gender markers in Afroasiatic (generally [unmarked] masculine / *t*-feminine); the resemblance was only in meaning, and thus irrelevant to genetic classification. The evidentiary value of the Nama gender markers (*b*-masculine / *s*-feminine) was dismissed in the same way, particularly since there are virtually no lexical cognates shared by Nama and Afroasiatic, while lexical and grammatical cognates abound between Nama and other Khoisan languages, and today most linguists accept the Khoisan affiliation of Nama. Simultaneously with his studies in genetic classification Greenberg (e.g., 1954b, 1957b, 1957c) was laying the foundations of modern linguistic typology, and he understood, possibly better than anyone else at the time, that typological classification is distinct from genetic classification.
B. **Mass comparison:** This remains the most widely misunderstood of the three fundamentals. Some linguists have mistakenly claimed that Greenberg tried to "bypass" the traditional historical linguistic methods, or that he meant mass comparison as a *substitute* for them, but nothing could be farther from the truth. Greenberg patiently explained that the usual tendency of comparing two languages at a time inevitably excludes much of the evidence for relationship that would appear when a wider selection of languages is consulted. This is seen to be obvious if one were to compare, say, only English,

Russian, and Hindi (the example Greenberg gave). With a broader sample of Eurasian languages, such as German, Italian, Greek, Persian, etc., the multiple lexical and grammatical strands connecting all these languages stand out in bolder relief. Well aware of the unfounded criticisms, Greenberg later changed the term to "multilateral comparison," and a more thorough discussion is found, for example, in Greenberg (1987: 1-37). Merritt Ruhlen (e.g. 1987, 1994, 2005) has repeatedly defended the procedure and demonstrated its effectiveness in comparing subsets of languages across just a few basic words, as Greenberg showed in the first chapter of *Languages of Africa*. Ruhlen (1987: 34) countered the common misconception that mass comparison was a new technique invented by Greenberg, since more than two centuries earlier Strahlenberg had used the mass comparison of basic vocabulary to classify the non-Indo-European languages of the Russian Empire with, as described by Roman Jakobson, "impressive exactitude." Manaster Ramer (1996) and Newman (2000: 262-3) emphasize the same point.

C. **Only linguistic evidence is relevant to genetic classification:** This seemingly obvious fundamental was emphasized by Greenberg in order to eliminate false notions such as the idea that cultural attributes like cattle herding and physical appearance of speakers had sometimes been used as criteria for classifying languages as "Hamitic" or "Nilo-Hamitic." Another aspect of this principle is that factors such as "practical importance, extent of population and territory or literary cultivation" must also be disregarded in genetic classification. The two major examples cited by Greenberg were Bantu, whose vast population and territorial expanse had obscured the fact that in strictly linguistic terms it was a small sub-subgroup of Niger-Congo; and Semitic, whose historical and literary prestige caused it to be overvalued as a taxon, whereas to Greenberg it was only one of five coordinate branches of Afroasiatic.[1] Elsewhere in the world some linguists (e.g. George van Driem) have argued that the vast population and cultural prestige of Chinese have artificially inflated its taxonomic position far beyond its strictly linguistic status as a subgroup of Tibeto-Burman.

Greenberg's fundamentals (A) and (C) seem by now to be generally accepted, while (B), multilateral comparison, remains controversial and misunderstood. Nevertheless,

> Greenberg's 1963 classification has formed the basis of all subsequent work on African classification. By eliminating the irrelevant nongenetic criteria that had vitiated all previous classifications, Greenberg demonstrated that the principles of genetic classification established in the nineteenth century for European languages were equally valid for African languages (Ruhlen 1987: 84).

Now, a half-century later, we pay tribute to Greenberg with articles and notes that evaluate, re-examine and update his work, or that have been inspired by it.

---

[1] Ehret's (2000) classification 'demotes' Semitic even further. In this scheme Semitic is a subgroup of a subgroup of a subgroup of Afroasiatic.

First, we mourn the passing of Christy G. Turner and Peter Michalove. Richard Scott has kindly provided us a commemoration of his colleague and friend, who in 1986 collaborated with Greenberg and Stephen Zegura in an influential article that sought to coordinate three lines of anthropological evidence, linguistic, dental, and genetic, in understanding the Pre-Columbian settlement of the Americas. Michalove, who shared musical as well as linguistic aptitude with Greenberg, contributed to research in Nostratic, a long range family (macrofamily) that overlaps to a large degree with Greenberg's Eurasiatic model.

Every historical linguist more or less vaguely knows about the (in)famous 'Paris Prohibition' of discussion of the origin of language (and, implicitly, of studying remote histories of languages), but probably few know how — and why — it originated. In the first article Pierre Bancel explores the origins of the Prohibition, the men who devised it, and their motivations.

In the next article Kirill Babaev provides an evaluation of Greenberg's Niger-Congo hypothesis, from a 'Moscow School' point of view. ASLIP founder Harold (Hal) Fleming's "Tribute and Appraisal" of Greenberg is reprinted from the 2001 issue of this journal. Roger Blench discusses the issue of why Africa, the homeland of humanity and most genetically diverse continent in the world, is home to only four language families, and no isolates, in Greenberg's scheme. Allan Bomhard, the leading American authority on the Nostratic hypothesis, offers a detailed outline of the Proto-Nostratic grammatical system, which coincides on many points with the Proto-Eurasiatic grammar postulated in Greenberg's (2000) book.

Hal Fleming and four colleagues have postulated a scenario for the dispersion of modern humans out of Africa, and throughout the rest of the world, correlating evidence from several anthropological disciplines, namely biological anthropology, archeology, and historical linguistics.

Following up on his article on Milyan nouns in our 2011 issue, Vitaly Shevoroshkin, an eminent member of the Moscow School, provides his analysis of Milyan verbs, from this extinct Anatolian language known only from inscriptions in stone from some 25 centuries ago. Our colleague in Bashkortostan, Shamil Nafiqoff, writes about Alfredo Trombetti, the 'father' of global etymologies and scholarly ancestor of Greenberg and other Long Rangers. Finally, we reprint the late Peter Michalove's report on the Moscow Conference on Long-Range Comparison (2000) from the 2001 issue of *Long Ranger* newsletter.

References

Ehret, Christopher. 2000. "Language and history." In Heine & Nurse (2000): 272-297.

Greenberg, Joseph H. 1948. "The classification of African languages." *American Anthropologist* 50: 24–30.

Greenberg, Joseph H. 1949-1950. "Studies in African linguistic classification: I; II; III; IV; V; VI; VII." *Southwestern Journal of Anthropology* 5: 79–100; 190–98; 309–17; 6: 47–63; 6: 143–60; 223–37; 388–98;

Greenberg, Joseph H. 1954a. "Studies in African linguistic classification: VIII. Further remarks on method; revisions and corrections". *Southwestern Journal of Anthropology* 10: 405–15.

Greenberg, Joseph H. 1954b. "A Quantitative Approach to the Morphological Typology of Language." In *Method and Perspective in Anthropology*, ed. R.F. Spencer, 192-220. Minneapolis: University of Minnesota Press.

Greenberg, Joseph H. 1955. *Studies in African Linguistic Classification*. New Haven: Compass Press. [Photo-offset reprint of the *SJA* articles with minor corrections.]

Greenberg, Joseph H. 1957a. *Essays in linguistics*. Chicago: University of Chicago Press.

Greenberg, Joseph H. 1957b. "Order of affixing: a study in general linguistics." In Greenberg 1957a, 86-94.

Greenberg, Joseph H. 1957c. "The nature and uses of linguistic typologies". *International Journal of American Linguistics* 23.2: 68–77.

Greenberg, Joseph H. 1963. *The Languages of Africa.* Bloomington: Indiana University Press / The Hague: Mouton & Co. [= Part II, *International Journal of American Linguistics* 29.1.]

Greenberg, Joseph H. 1966. *The Languages of Africa.* Bloomington: Indiana University Press / The Hague: Mouton & Co. [2nd ed. with additions and corrections.]

Greenberg, Joseph H. 1971. "The Indo-Pacific hypothesis." *Current Trends in Linguistics, Volume 8: Linguistics in Oceania*, ed. T.F. Sebeok, 807-871. The Hague: Mouton.

Greenberg, Joseph H. 1987. *Language in the Americas*. Stanford: Stanford University Press.

Greenberg, Joseph H. 2000. *Indo-European and Its Closest Relatives: The Eurasiatic Language Family, Volume 1: Grammar*. Stanford: Stanford University Press.

Greenberg, Joseph H. 2002. *Indo-European and Its Closest Relatives: The Eurasiatic Language Family, Volume 2: Lexicon*. Stanford: Stanford University Press.

Greenberg, J.H., Turner, C.G., II, and Zegura, S. 1986. "The settlement of the Americas: a comparison of the linguistic, dental, and genetic evidence." *Current Anthropology* 27: 477-497.

Heine, Bernd, & Derek Nurse, ed. 2000. *African Languages: An Introduction*. Cambridge University Press.

Manaster Ramer, A. 1996. "Tonkawa and Zuni: Two Test Cases for the Greenberg Classification." *International Journal of American Linguistics* 62.3: 264-288.

Newman, Paul. 2000. "Comparative linguistics." In Heine & Nurse (2000): 259-271.

Ruhlen, Merritt. 1987. *A Guide to the World's Languages: Volume 1: Classification*. Stanford: Stanford University Press.

Ruhlen, Merritt. 1994. *The Origin of Language*. New York: John Wiley.

Ruhlen, Merritt. 2005. "Taxonomy, Typology and Historical Linguistics," in *Language Acquisition, Change and Emergence*, ed. J.W. Minnet & W.S.-Y.Wang, 341–68. Hong Kong: City University of Hong Kong Press.

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

In Memoriam

Christy G. Turner II

November 28, 1933 – July 27, 2013

G. Richard Scott
University of Nevada Reno[1]

I was an undergraduate at Arizona State University in 1966 when Christy Turner was hired into a rapidly expanding Department of Anthropology. I took three of his courses as an undergraduate: World Prehistory, Southwest Archaeology: Anasazi, and Physical Anthropology (Human Biology). He was a challenging and charismatic professor who slowly brought me into the 'physical anthropology' fold, redirecting me away for my original goal of becoming an archaeologist. He talked Charles Woolf into offering me a graduate fellowship in genetics and that dictated my direction in graduate school, the genetic analysis of dental morphological traits.

In 1969, I recall seeing a mimeograph of an abstract that Christy prepared for a national meeting. It involved the use of dental morphology to make inferences on the origins of Native American populations. He had zeroed in on one trait that he thought was telling – 3-rooted lower first molars. In his dissertation *The Dentition of Arctic Peoples* (1967; reprinted in 1991) he reported an exceptionally high frequency of this trait in Eskimos and Aleuts (30-40%). When he later examined American Indian samples from the Southwest and California, he found the trait to be much less common (ca. 5%). While this corresponded to the long held biological distinction between Eskimo-Aleuts and American Indians, he also noted a 27% frequency in a living Navajo sample that fell between these two large groupings. From this he concluded that "Three migrations into the New World seem to best explain 3RM1 variation in this hemisphere. Pre-Indians, pre-Na-Dene Indians, and pre-Aleut-Eskimos are the three suggested ancestral groups. Importantly, these coincide with major New World linguistic divisions recognized by Greenberg and Swadesh" (Turner, 1971:239).

That one extra root on the lower molars ultimately led to a collaboration that produced the seminal article *The Settlement of the Americas: A Comparison of the Linguistic, Dental, and Genetic Evidence* by Joe Greenberg, Christy, and Steve Zegura (1986). By the mid-1980s, Christy had traveled extensively throughout the Americas and Asia to study thousands of skeletons in dozens of museums. After amassing an enormous data set on 29 dental traits, he found the same pattern suggested by the three-rooted lower first molar. I was teaching in Alaska by this time so I was not privy to how Christy and Joe Greenberg decided to publish an article together, but it led to a significant result. The three-wave model proposed in the article based on a combination of linguistic, dental, and genetic data has been the benchmark for hundreds of publications on the peopling of the Americas over the past 28 years. Initially, the premise of the article was widely accepted by archaeologists, physical anthropologists, and geneticists, but then came under fire by researchers studying mitochondrial DNA and Y chromosome haplogroups. Ironically, a recent meta-analysis of genetic data by David Reich and 63 collaborators (2012) demonstrated that the burial of the three-wave model may have been premature. In concluding remarks, they note "the three distinct Asian lineages we detect – 'First American', 'Eskimo-Aleut', and a separate one in the Na-Dene-speaking Chipewyan – are consistent with a three-wave model proposed mostly on the basis of dental morphology and a controversial interpretation of the linguistic data" (Reich et al., 2012:373).

---

[1] Department of Anthropology, University of Nevada Reno, Reno NV 89557.

When I defended my dissertation in the summer of 1973, I was Christy's first PhD in physical anthropology. Through his Wisconsin contacts, he helped me secure a job at the University of Alaska Fairbanks. Although interior Alaska was not my first choice of destinations, the doors of academia were closing at that time so I took the job and came to appreciate the opportunities it provided. For one, the Anthropology department had close ties with the Alaska Native Language Center just one floor below, so through interaction with Michael Krauss, Jim Kari, Larry Kaplan, and others, it heightened my appreciation for how language could be used to reconstruct population history. Not to mention it put me in the heart of linguistic efforts on two of the three languages in the Greenberg-Turner model, Na-Dene and Eskimoan.

After leaving for Alaska, Christy and I stayed in close touch and collaborated on many articles. Neither of us ever thought much about writing books, but in the 1990s, at Gabe Lasker's invitation, we wrote *The Anthropology of Modern Human Teeth: Dental Morphology and Its Variation in Recent Human Populations* (1997), published by Cambridge University Press. In chapter five, we laid out world variation for 23 dental morphological traits. In coming up with a method to organize the data, language played a major part. From the beginning to the end of his career, Christy was first and foremost an anthropologist who used linguistic and archaeological information to help interpret patterns of biological variation. When the book drew near completion, we were talking about titles. I originally suggested only the second half of the title. It was Christy who thought 'The Anthropology of Modern Human Teeth' had more panache, and was often the case, he was absolutely right.

The traits that best describe Christy are driven, productive, imaginative, and far sighted. While others described dental morphological traits, Christy could see their potential in asking questions of population origins and relationships. He travelled the world over and made dental observations on over 30,000 human skeletons. He knew the insides of about every museum in North America and many in South America, Siberia, North Asia, Southeast Asia, and Europe. But he didn't just describe teeth; he developed the methods that are the foundations of modern dental comparative studies (The Arizona State University Dental Anthropology System; Turner et al., 1991). Beyond methods, he developed models that addressed long-standing historical issues, including not only the three-wave model for the peopling of the Americas, but also the dental division between North Asians (Sinodonts) and Southeast Asians (Sundadonts) and the origins of Pacific populations (Turner, 1990). While his ideas may or may not prove to be correct in the long run, he developed models that other researchers had to consider, even if they disagreed with him.

On another front, Christy started what was basically a new field of inquiry when he examined what was presumed to be a secondary burial from Polacca Wash, a site near the abandoned Hopi village of Awatovi. Although his interest, per usual, was in making dental observations, he thought this collection of broken and burned bones could hardly be a secondary burial. After a detailed analysis of cut marks, anvil abrasions, burned bone, etc., he wrote his first paper on Southwest cannibalism entitled "A Massacre at Hopi" (Turner and Morris, 1970). After examining this series, he started an exhaustive search for more skeletal collections from the Anasazi region that could be best explained by cannibalism. Needless to say, this was not a popular view and he was attacked by Native Americans and professional colleagues who took issue with his interpretation. The culmination of this work came in the volume *Man Corn: Cannibalism and Violence in the Prehistoric American Southwest* (1999), co-authored with Jacqueline, his wife, companion, and collaborator. Always thorough to an admirable fault, he went to central Mexico and examined skeletal series where cannibalism was widely acknowledged. His view was that if you disagreed with his taphonomic signature for cannibalism, provide an alternative explanation for bones that had been bashed, burned, and butchered. Christy had file after file of papers written on the subject and he never felt anyone seriously dented his interpretation of cannibalism, which he always clearly separated from evidence of just violence.

Christy was definitely the hardest working scholar I have ever known. With failing health and eyesight greatly diminished by macular degeneration, he completed another book for Cambridge University Press entitled *Animal Teeth and Human Tools: A Taphonomic Odyssey in Ice Age Siberia.* Thankfully, he finished all the edits and copy proofing of the galleys in the spring of 2013 but, unfortunately, the volume came out shortly after his death. How he could write and edit a 500 page volume with dozens of tables and photos with all of his physical limitations astounds me to this day. He had a great career and had already made significant marks in dental anthropology and the taphonomy of human cannibalism but he kept pressing on.

When I was a graduate student, Christy told me his academic idols were William Healy Dall, Franz Boas, and Aleš Hrdlička, who published, respectively 1500, 800, and 600 papers and books. Christy did not quite match these gentlemen but it was not for lack of effort. He published four books and approximately 160 articles. A citation analysis shows he had 13 items cited over 100 times, 47 over 20 times, and 64 over 10 times. The total number of citations stands around 4400 and will continue to expand into the foreseeable future. When you break ground in multiple areas, you leave a big scholarly footprint. His legacy is substantial and he will be missed and remembered by his family and dozens of former students and colleagues because he was a 'unique character' whose time on earth made the world a more interesting place.

References

Greenberg, J.H., Turner, C.G., II, and Zegura, S. 1986. The settlement of the Americas: a comparison of the linguistic, dental, and genetic evidence. *Current Anthropology* 27:477-497.

Reich, D., Patterson, N., Campbell, D. et al. 2012. Reconstructing Native American population history. *Nature* 448:370-375.

Scott, G.R. and Turner, C.G. II. 1997. *The Anthropology of Modern Human Teeth: Dental Morphology and Its Variation in Recent Human Populations.* Cambridge: Cambridge University Press.

Turner, C.G., II. 1971. Three-rooted mandibular first permanent molars and the question of American Indian origins. *American Journal of Physical Anthropology* 34:229-242.

Turner, C.G., II. 1990. Major features of Sundadonty and Sinodonty, including suggestions about East Asian microevolution, population history, and late Pleistocene relationships with Australian aboriginals. *American Journal of Physical Anthropology* 82:295-317.

Turner, C.G., II. 1991. *The Dentition of Arctic Peoples.* New York: Garland Publishing, Inc.

Turner, C.G., II, and Morris, N.T. 1970. A massacre at Hopi. *American Antiquity* 35:320-331.

Turner, C.G., II, Nichol, C.R. and Scott, G.R. 1991. Scoring procedures for key morphological traits of the permanent dentition: the Arizona State University dental anthropology system. In *Advances in Dental Anthropology*, eds. M.A. Kelley and C.S. Larson, New York: Wiley-Liss, pp. 13-31.

Turner, C.G., II and J.A. Turner 1999. *Man Corn: Cannibalism and Violence in the Prehistoric American Southwest.* Salt Lake City: University of Utah Press.

Turner, C.G., II, Ovodov, N.D. and Pavlova, O.V. 2013. *Animal Teeth and Human Tools: A Taphonomic Odyssey in Ice Age Siberia.* Cambridge: Cambridge University Press.

Peter A. Michalove
March 22, 1951 - December 9, 2013[2]

Peter A. Michalove was born March 22, 1951 in Greensboro, N.C. When he was ten years old an uncle gave him a little plastic recorder (woodwind instrument) that everyone in his family called a 'flute'. This was his first exposure to music, and the beginning of much more to come.

Peter started writing music for the flute and, eventually, other instruments. He got books from the public library on harmony, orchestration, and, eventually, counterpoint. Most students hated music theory, but he loved it. At the age of 13 he wrote a piece for band, and his junior high band director agreed to have the high-school band play it. By this time, at the age of 14, he had found his main calling in life as a composer.

Peter began undergraduate studies at the University of North Carolina at Chapel Hill. After getting a bachelor's degree at Chapel Hill, he went to the University of Michigan at Ann Arbor for the master's. He then applied to the University of Illinois for a doctorate, and was accepted. In Champaign-Urbana he had two excellent teachers (Salvatore Martirano and Ben Johnston), and there he also met Sharon Deborah Grodsky. Within six weeks they were engaged, and married after nine months.

But when Peter finished music school, as a Doctor of Musical Arts in composition, he did not get a job, but acquired a degree in accounting and spent most of a 30-year career doing administrative work at Champaign-Urbana. He wrote music sporadically, but there hardly seemed any reason to write to an audience that wasn't there.

Aside from music another of Peter's major interests was in languages. In addition to his native tongue, Peter was fluent in German, Russian, and French. He learned Turkish, Czech, Italian, Classical Mongolian (see Michalove 2004b), Latin, Icelandic, and Greek. For several years in the late 1990s Peter expanded on his interest to include studies of Altaic, Uralic, and their putative ancestor, Nostratic. In 2001 the ASLIP newsletter *Long Ranger* published his report on the conference "Problems in the Study of Long-Range Linguistic Comparison at the Turn of the Third Millennium" in Moscow, May-June 2000, which is reprinted in this issue. His other works in historical linguistics and Nostratic studies, insofar as they are known to *Mother Tongue*, are listed below. After 2004 Peter seems to disappear from the linguistic scene, apparently due to the revival of his first love, composing music.

Peter had many other interests. In 1988 Peter and David Skipton produced the two volume *Russian Postal Censorship to 1920*, an historical and philatelic study covering the background of political, social, civil and military censorship in Imperial Russia.

Peter was also an amateur "stand-up comic," as a friend relates, "in his mild-mannered way, he loved being 'on', and had a wonderful pixilated sense of humor … If you were not careful, you could inadvertently wind up as his straight man (or woman). He could not resist picking the low-hanging fruit, is the way he put it. He was a master opportunist when it came to the pun. It delighted him that punning was considered the lowest form of humor."

---

[2] We are indebted to Sharon Michalove for sharing Peter's obituary and other materials. [Ed.]

In 2004 Peter discovered a free on-line music notation software program. From there he began writing music, and he did not stop until quite recently. He retired from the University of Illinois in 2006 and began composing in earnest. In 2008 Peter was diagnosed with an extremely aggressive case of prostate cancer. By some time in 2013, he had lost the stamina to compose any more. On Feb 8, 2013, a concert of his work was given at the Indi-Go Artist's Co-op in Champaign.

Peter shared the progression of his illness on Facebook and on his blog, and near the end wrote his own obituary, part of which is paraphrased here. Peter died on December 9, 2013.

Memorial contributions may be made to the Prostate Cancer Fund at pcf@org.com or to the University of Illinois Foundation, designated on the check in memory of Peter Michalove and sent to the attention of Jeff Fehrenbacher.

Peter A. Michalove: Linguistic Bibliography

Michalove, Peter A. 1999. "The treatment of initial *l- in Proto-Samoyed." *Journal de la Société Finno-Ougrienne* 89.

Michalove, Peter A. 2000. "The Nostratic theory and the development of word-final velars in Indo-European." In: *Problemy izučenija dal'nego rodstva jazykov na rubeže tretjego tysjačiletija [Problems in the Study of Long-Range Linguistic Comparison at the Turn of the Third Millennium].* Doklady i tezisy naučnoj konferencii [Reports and handouts of the scientific conference], ed. Sergei Starostin, pp. 206-215. Moskva: Russkij gosudarstvennyj gumanitarnyj univerzitet [Russian State University for Humanities].

Michalove, Peter A. 2001. "Notes on the Moscow Conference on Long-Range Comparison." *Long Ranger* (Newsletter of the Association for the Study of Language in Prehistory) 33, part 1. September 2001.

Michalove, Peter A. 2002a. "The Nostratic 'Accusative' in *-mA: An Altaic Perspective." *Anthropological Linguistics* 44.1: 85-95.

Michalove, Peter A. 2002b. "The Role of Morphology in Nostratic Studies." In: Shevoroshkin & Sidwell 2002: pp. 13-17.

Michalove, Peter A. 2002c. "The Role of Ockham's Razor in Historical Linguistics." In: Cavoto 2002: pp. 303-308.

Michalove, Peter A. 2002d. "The Classification of the Uralic Languages: Lexical Evidence from Finno-Ugric." *Finnisch-Ugrische Forschungen* 57: 58-67.

Michalove, Peter A. 2004a. "Vowel Harmony and Other Forms of Vocalic Assimilation in Mongolic." In: Hegedűs & Sidwell 2004: pp. 165-174.

Michalove, Peter A. 2004b. *A Guide to Reading Mongolian Texts in the 'Phags-pa Script.* (The Mongolia Society Special Papers Issue 15, Fall 2004.) Bloomington, Ind.: The Mongolia Society.

Michalove, Peter A. 2005. Review of Keith W. Slater, *A Grammar of Mangghuer: A Mongolic Language of China's Qinghai-Gansu Sprachbund. Diachronica* 22.1: 213.

With co-authors / co-editors:

Georg, Stefan, Peter A. Michalove, Alexis Manaster Ramer, & Paul J. Sidwell. 1999. "Telling general linguists about Altaic." *Journal of Linguistics* 35: 65-98.

Hegedűs, Irén, Peter A. Michalove, & Alexis Manaster Ramer (Eds.). 1997. *Indo-European, Nostratic, and Beyond: Festschrift for Vitalij V. Shevoroshkin*. (JIES Monograph No. 22.) Washington D.C.: Institute for the Study of Man.

Manaster-Ramer, Alexis, Peter A. Michalove, Karen S. Baertsch & Karen L. Adams. 1998. "Exploring the Nostratic Hypothesis." In: Salmons & Joseph 1998: 61-84.

Manaster Ramer, Alexis, & Peter A. Michalove. 2009. "Methodologies in long-range comparison and the reconstruction of Nostratic." (Review article of Bomhard & Kerns 1994.) *Linguistics* 35.3: 589-596.

Michalove, Peter A., Stefan Georg, & Alexis Manaster Ramer. 1998. "Current Issues in Linguistic Taxonomy." *Annual Review of Anthropology* 27: 451-472.

Michalove, Peter A., & Alexis Manaster Ramer. 1999. "The Use of Reconstructed Forms in Nostratic Studies." In: Renfrew & Nettle 1999: pp. 231-242.

Books cited:

Bomhard, Allan R., & John C. Kerns. 1994. *The Nostratic Macrofamily: A Study in Distant Linguistic Relationship*. Berlin, New York, NY, & Amsterdam: Mouton de Gruyter.

Cavoto, Fabrice (Ed.). 2002. *The Linguist's Linguist: A Collection of Papers in Honor of Alexis Manaster Ramer.* Munich: LINCOM EUROPA.

Hegedűs, Irén, & Paul Sidwell (Eds.). 2004. *Nostratic Centennial Conference: The Pécs Papers*. Pécs: Lingua Franca Group.

Renfrew, Colin, & Daniel Nettle (Eds.). 1999. *Nostratic: Examining a Linguistic Macrofamily*. Cambridge, Eng.: The McDonald Institute for Archaeological Research.

Salmons, Joseph C., & Brian D. Joseph (Eds.). 1998. *Nostratic: Sifting the Evidence*. Amsterdam / Philadelphia: John Benjamins.

Shevoroshkin, Vitaly, & Paul J. Sidwell (Eds.). 2002. *Languages and Their Speakers in Ancient Eurasia. Dedicated to Professor Aharon Dolgopolsky on his 70th Birthday.* (= AHL Studies in the Science of History of Language, vol. 5.) Melbourne: Association for the History of Language.

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

Move along, there's nothing to see here: How the SLP ban was pronounced

Pierre J. Bancel*f*#

The appearance of *The Languages of Africa* (Greenberg 1963) is arguably the most important event of the 20th century in comparative-historical linguistics, for the powerful new beginning it gave to the study of remote linguistic relationships. The field had remained nearly dormant for the preceding hundred years, while the few dissenting attempts had been heavily barraged by prominent linguists. In this special issue of *Mother Tongue* celebrating the fiftieth anniversary of its publication, it may be appropriate to return to the distant event that inaugurated the blockade.

This event is so well known and has been quoted so often that one might doubt that there be any point even in mentioning it. However, the reasons behind it have never been clearly explained. In 1866, as probably no linguist today ignores, the newly constituted Société de linguistique de Paris (SLP) enshrined in its Statutes, article 2, that

> [t]he Society does not admit any communication concerning … the origin of language.
>
> (Société de linguistique de Paris 1871: III; our translation)

This ban did not remain particular to French linguistics. It was widely approved and upheld around the world, with the consequence that for more than a century this subject nearly disappeared from linguistic circles. Moreover, no difference was made at the time between the origin of *language* and that of *languages* – while we know today that perhaps several million years may have elapsed between the first hominids who took the path toward articulate speech and the most ancient language ancestral to all known languages. Proto-Sapiens, as it has come to be known, is obviously no older than our biological species, *Homo sapiens*, which appeared between 250,000 and 200,000 years before present (yBP). Moreover, its most ancient version we can hope to reach through comparison may date back approximately to the first exit of Sapiens humans from their African cradle, around 100,000 yBP.

---

f French Translation Section, United Nations Office at Geneva (Switzerland); the views expressed here are those of the author and do not necessarily reflect those of the United Nations. # Association d'études linguistiques et anthropologiques préhistoriques (Paris). Thanks to Shahar Fineberg for his insightful revision of this article. Mail to: pierrejbancel@hotmail.com.

As a result, both issues were then consistently considered to be one and the same thing, a confusion still made today, e.g. by Sylvain Auroux, stating that

> the most fertile century in studies on the *origin of language* is undoubtedly the nineteenth. Most of the great names in linguistics (W. von Humboldt, F. [von] Schlegel, J. Grimm, A. Schleicher, A. [read: H.] Steinthal, H. Paul, E. Renan, M. Muller [read: Müller], D.G. Withney [read W.D. Whitney], M. Bréal) brought their contribution, among a host of lesser known authors whose works are only partly reflected in our bibliography. The development of linguistics – the clear awareness that this was a "new science" based on the knowledge of facts and on a new method (comparing languages) – did not extinguish the project to resolve the question of the *origin of languages*.
> (Auroux 2006: 59–60; our translation, *emphasis*, and [interpolations])

Throughout the rest of his article, Auroux does not allude once to the difference between the origin of language and that of languages, which seems to escape him completely. Consistent with this confusion, the last fourth of this paper is an attack, as harsh as it is ill-informed,[1] against Joseph Greenberg's (1987) Amerind book, and above all against John Bengtson and Merritt Ruhlen's (1994) Proto-Sapiens roots. While Bengtson & Ruhlen (1994: 277-278) explicitly claim that these roots are to be traced back to a mother tongue of all known languages, and not to the origin of language, Auroux concludes his rejection saying that

> [t]he theoretical consequences [read: basis] of the ban proclaimed by the Société de linguistique [de Paris] are infinitely firmer than what may be reckoned from a few booming claims of the last thirty years. (Auroux 2006: 85; our translation)

As it appears, the SLP ban certainly was the first official manifestation of the refusal to even consider remote linguistic relationships, which later in the 19th and the early 20th centuries came to be theorized in the form of the Ultra-Neogrammarian credo.

Let us, then, examine the "theoretical basis" of this ban which, while officially removed ten years later from the SLP new Statutes, still finds linguists to defend it today.

---

[1] Auroux (2006: 82) claims that Greenberg and Bengtson & Ruhlen have abandoned the phoneme-by-phoneme comparison, the essential methodological conquest of 19th century linguists, and returned to a prescientific word-by-word comparison. Indeed, both Greenberg and Bengtson & Ruhlen have explained repeatedly, including in the very works criticized by Auroux (Greenberg 1987: 1–37; Bengtson & Ruhlen 1994: 279–281) that, while they do not regard as necessary, at the taxonomic stage, the recurrence of phonetic correspondences across etymological series, which are eroded more quickly than vocabulary itself by various phenomena (word loss, analogy and other unsystematic factors of change), they systematically restrain their comparisons to plausible (i.e. well-attested in undisputed families) phoneme-by-phoneme correspondences.

The academic legend: preserving science from empty speculations

It was recounted until recently to students in linguistics that the SLP ban was pronounced because of the many unfounded theories then put forward about the origin of language. It thus essentially aimed at sparing the precious time of linguists in avoiding endless and pointless discussions of a problem to which no satisfying solution was technically possible.

This justification was taught to the first author in the first year of his linguistic studies in France during the 1980s. It is also mentioned, without reference to a written source, as if it were commonplace, by non-linguists interested in language evolution, e.g. Merlin Donald (1991: 25) or Terrence Deacon (1997: 14). The distinguished long-ranger Merritt Ruhlen himself also believed it recently (1991: 262). It may be regarded as an academic legend, and anyway bears very little relation to historical truth, as will be seen below.

Regarding its general relevance, one might wonder whether the duty of scientists, when faced with stories they judge ill-founded or insane about the very object of their discipline, would not precisely be to relentlessly investigate the question until they came up with a satisfactory answer, rather than forbidding discussion of it in order to spare their time and peace of mind.

Nor are there evident traces of a proliferation of speculations about language origins in the first part of the 19th century. It is worth noting that seven out of the ten authors mentioned by Auroux in his first quotation above, namely Friedrich Schlegel, Jacob Grimm, August Schleicher, Hermann Paul, Max Müller, William Dwight Whitney and Michel Bréal, are well known and remembered for their important contributions to the development of scientific comparative linguistics, essentially within the Indo-European domain, rather than for any empty speculations about the emergence of speech in humans – nor for any claims that all languages would be related.

Many speculations on the origin of articulate speech, however, had been put forward in the previous century by authors of the Enlightenment – people as irrelevant to the history of human thought as the Englishmen John Locke and Adam Smith, the Frenchmen Etienne de Condillac and Jean-Jacques Rousseau, or the Germans Gottfried Wilhelm Leibniz, Johann Gottlieb Fichte and Wilhelm von Humboldt,[2] to cite a few. Their reflections, which in many respects prefigured an evolutionary approach, of course lacked the tools forged by the 19th century comparativists, but offered precisely the opportunity to test the strength of these tools at a higher level.

Why did the SLP choose not to take the challenge?

---

[2] See Locke (1690), Smith (1767), Condillac (1746), Rousseau (1781), Leibniz (1705), Fichte (1795), Humboldt (ca. 1795).

The original SLP recipe: one half obscurantism, one half colonialist racism, add adventurism to taste

Why? Once the question is correctly posed, the answer becomes almost obvious. Why, a few years after the publication of Darwin's (1859) *On the Origin of Species*, at the height of the turmoil caused by the radical challenge it had laid down to a literal reading of the biblical Genesis, why prohibit studying such an intrinsically evolutionary phenomenon as the origin of language ability in humans? Out of religious prejudice? Indeed.

Why, in a time when the European powers, barely through with three centuries of slave trade and exploitation, were strengthening their grip on the entire planet, colonizing all continents in order to allegedly bring the benefits of civilization to "inferior" peoples, why refuse to consider the source of the universality of the most characteristic phenomenon of human culture – articulate language –, known and practiced by all peoples and placing them all on exactly the same level, which should, logically, spare their being civilized and dispossessed by cannon fire? Out of political and racial prejudice? Indeed.

These are not gratuitous indictments. A long-term SLP official, Gabriel Bergounioux is little suspect of malignancy towards the Société, and the SLP website itself displays his "Historique de la Société de linguistique de Paris", which begins with the following paragraph:

> The Société de linguistique de Paris (SLP) appears around 1863. It is then a discussion circle where a few Americanists meet, having parted from the Société d'ethnographie orientale et américaine because of personal quarrels. Their intention is to set up, against the Société d'anthropologie de Paris founded in 1859 by Broca and inspired by Chavée's materialist work, a competing society, close to Monarchist and Catholic circles and which would rely on the study of languages to voice their opinions.
>
> (Bergounioux no date: § 1; our translation)

Now, one may be reminded that Paul Broca, the discoverer of the famous Broca's language area in the human brain (Broca 1861), was also, through the Société d'anthrolopologie de Paris (SAP) he had founded, the main proponent in France of Darwin's evolutionary theory. It clarifies what these Catholic Americanists had in mind when they wanted to compete with Broca and the SAP and "voice their opinions." To them, the inherently evolutionary question of the origin of language was not a matter *at*, but *for the* stake.

The Anti-Darwinian stance of the SLP founders is confirmed by Bergounioux in two other articles, both published in the *Bulletin de la SLP*. In the first, he notes that

> [th]e Société d'ethnographie américaine et orientale [which the SLP founders had originated from] … sides with the official, conformist France. … No hypotheses contrary to theories recognized by the State and the Church are put forward there. … [Members]

> have in common to refuse … evolutionism, and more generally materialism.
> (Bergounioux 1996: 5; our translation)

This is not all. In the same article, Bergounioux also discreetly explains that the first president of the SLP, Count Hyacinthe de Charencey, seemingly a well-off gentleman, invested a lot of money in the attempted conquest of Mexico by Napoléon III and his puppet Maximilian I:

> A representative of the Catholic wing of the Imperial Party, and a Peer of France, [de Charencey] committed part of his personal wealth to the service of his ideas. The dream of a Catholic-Latin Empire of America seduced him …
>
> (Bergounioux 1996: 8; our translation)

In his second study, Bergounioux confirms all of these facts, still with a sense of discretion:

> No, the SLP was not an assembly of scientists willingly gathered to contribute to the progress of knowledge … It was born from a circle of *amateurs polygraphes* ["write-about-many-things amateurs"], Conservative Catholics close to the Imperial power. Their project fits into the framework of a confrontation with Positivist, Darwinian Republicans.
> …
> The Americanist tropism of some members, in particular Hyacinthe de Charencey, seems to be not without relation to the Mexican adventure of Napoléon III.
> (Bergounioux 2005: 361–362; our translation)

This produces a rather different picture from that of the poor overworked scientists who could no longer stand spending their nights sorting out the deluge of rubbish poured out by Condillac's or Leibniz's imagined epigones.

To put it bluntly, the prohibition against studying the origin of language was initially determined by a coterie of reactionary, bigoted, obscurantist adventurists disguised as *Sorbonnards*. They were certainly nothing like an assembly of good willed scientists, and what little linguistic work they may perhaps have produced has fallen into complete oblivion. As a result, for a hundred years, linguists prevented themselves from thinking about what should have been from the beginning two of the main goals of their science – discovering the remotest historical relationships between languages and explaining the evolution of the speech capacity in humans – because of the prejudices and personal interests of this less than worthy clique.

From the history of languages to the evolution of language

Linguistics as a science was largely created, and its basic tools shaped, in the effort launched in the early 19th century to uncover the history of languages and their genetic relationships – how they evolved from ancestor languages by descent with modification.

As such, linguistics is an evolutionary discipline by birth, and even preceded biology on this scientific path, as remarked by Ruhlen (1994: 1).

Starting with African languages and taking stock of the practical and theoretical successes gained through comparison of more recent language families, Greenberg made it once again possible to reconstruct the remotest history of languages on a scientific basis.[3]

Neither Proto-Eurasiatic, Proto-Amerind nor even Proto-Sapiens represent any ultimate origin of language (something which, by the way, certainly never existed, just as there never was a first bird, a first mammal, or, for that matter, a first human being). But they are observatories, whose differences with their modern descendants may indicate which evolutionary steps were last crossed in the development of articulate language, thus indicating the path towards understanding how an ape lineage ended up human. In this regard, our argument that the definite absence of any clearly identifiable Proto-Sapiens 1st and 2nd person marker, contrary to all expectations raised by their exceptional preservation in all lower-level taxa, represents a first attempt at characterizing the evolutionary stage of Proto-Sapiens (Bancel & Matthey de l'Etang 2008: 457-462, 2013: 362–364).

Seen from their Anti-Darwinian perspective, the founders of SLP were wise to confuse the origin of language with that of languages, after all. Actually, there is no hope of uncovering anything important about the former without knowledge of the latter. Barring the study of remote relationships between languages, besides preventing the recognition that humans of all continents are very closely related, also was an efficient means to prevent that any light be shed by linguistics upon the "descent of man."

Greenberg, who restored, together with the historical perspective, the scientific dignity of the discipline, will be remembered as a beacon in a century of darkness.

References

AUROUX S. 2006. Les embarras de l'origine du langage. *Marges linguistiques* **11**, 58–92.

BANCEL P. J. & A. MATTHEY DE L'ETANG. 2008. The millennial persistence of Indo-European and Eurasiatic pronouns and the origin of nominals. In J. D. BENGTSON (ed.), *In hot pursuit of language in prehistory. Essays in the four fields of anthropology in honor of Harold Crane Fleming*, 439–464. Amsterdam: Benjamins.

BANCEL P. J. & A. MATTHEY DE L'ETANG. 2013. Brave new words. In C. LEFEBVRE, H. COHEN & B. COMRIE (eds.), *New perspectives on the origins of language*, 333–377. Amsterdam: Benjamins.

BENGTSON J. D. & M. RUHLEN. 1994. Global etymologies. In RUHLEN 1994.

---

[3] See in particular his writings on linguistic taxonomy compiled by William Croft in Greenberg (2006).

BERGOUNIOUX G. No date. Historique de la SLP. Online http://www.slp-paris.com/.

BERGOUNIOUX G. 1996. Aux origines de la Société de linguistique de Paris (1864–1976). *Bull. Soc. ling. Paris* **91**–1, 1–36.

BERGOUNIOUX G. 2005. La Société de linguistique de Paris dans son histoire (1863–1970). *Bull. Soc. ling. Paris* **100**–1, 359–390.

BROCA P. 1861. Sur le principe des localisations cérébrales. *Bull. Soc. anthr. Paris* **2**, 190–204.

CONDILLAC É. BONNOT DE. 1746. *Essai sur l'origine des connoissances humaines*. Deuxième Partie, *Du langage et de la méthode*, 1–24. Amsterdam: Mortier.

DARWIN C. 1859. *On the origin of species by means of natural selection, or the preservation of favoured races in the struggle for life*. London: Murray.

DEACON T. W. 1997. *The symbolic species. The co-evolution of language and the brain.* New York: Norton.

DONALD M. 1991. *Origins of the modern mind. Three stages in the evolution of culture and cognition*. Cambridge, MA: Harvard U. Pr.

FICHTE J. G. 1795. Von der Sprachfähigkeit und dem Ursprung der Sprache. In F. I. NIETHAMMER (ed.), *Phil. J. Gsllsch. teutscher Gelehrten*, **1**-3. Neu-Strelitz: Michaelis.

GREENBERG J. H. 1963. *The languages of Africa.* Bloomington: Indiana U. Pr.

GREENBERG J. H. 1987. *Language in the Americas.* Stanford: Stanford U. Pr.

GREENBERG J. H. (ed. by W. CROFT). 2006. *Genetic linguistics. Essays on theory and method.* Oxford: Oxford U. Pr.

GRIMM J. 1819 [2nd ed. 1822]. *Deutsche Grammatik, 1. Theil*. Göttingen: Dieterich.

HUMBOLDT W. VON. 1903–1936 (written ca. 1795). *Gesammelte Schriften*, 7. Bd, 581–583. Berlin: Pr. Akad. der Wschften.

LEIBNIZ G. W. 1765 (wr. ca. 1705). *Nouveaux essais sur l'entendement humain*. Leipzig: Raspe.

LOCKE J. 1690. *An essay concerning human understanding.* London: Basset.

ROUSSEAU J.-J. 1781. *Essai sur l'origine des langues*. Genève: Soc. hist. arch. Gen. [Paris: L'Harmattan, 2009].

RUHLEN M. 1991 [2nd rev. ed.]. *A guide to the world's languages*, vol. I: *Classification*. Stanford: Stanford U. Pr.

RUHLEN M. 1994. *On the origin of languages. Studies in linguistic taxonomy*. Stanford: Stanford U. Pr.

SMITH A. 1767. *The theory of moral sentiments, to which is added A dissertation on the origin of languages*, 437–478. London/Edinburgh: Millar, Kincaid & Bell.

SOCIÉTÉ DE LINGUISTIQUE DE PARIS. 1871. Statuts approuvés par décision ministérielle du 8 mars 1866. *Bull. Soc. ling. Paris* **1**-1, III–V.

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

Joseph Greenberg and the Current State of Niger-Congo

Kirill Babaev

Russian State University for the Humanities, Moscow

The year 2013 was the 50th anniversary of Greenberg's fundamental book [1963] which marked the beginning of a new era in African linguistics. Since then, any linguist working in this field should have inevitably defined himself as 'pro-Greenberg' or 'anti-Greenberg', but the overall opinion of the linguistic community about Greenberg's four macrofamilies of Africa has been quite positive. The elegance of the theory has captured many hearts in the community which otherwise did not have a clue of how to deal with hundreds and hundreds of African languages seeming so dissimilar to each other. The four macrofamilies of Greenberg gave an easy and comfortable basis for classification.

Today, after 50 years, Greenberg's theory is more often criticized than supported. Monographs and articles dealing with genetic grouping of the languages of Africa tend either to revise or completely deny the four macrofamilies of Greenberg as an obsolete, methodologically incorrect and factually inaccurate version. Moreover, the farther we go in time from Greenberg's book, the more legends start to appear about his activities, views and research. This paper aims to identify and possibly clarify some of these legends of contemporary African linguistics, as well as to propose a proper way to avoid these in the future.

1. Greenberg's Revolution

The 1963 work (and its early versions starting to appear since 1949) is sometimes regarded as a revolutionary view on the genetic classification of African languages, which was elaborated by Greenberg from scratch and drastically changed the landscape of African linguistics.

This idea is quite incorrect. In reality, in many aspects of his theory, Greenberg followed earlier hypotheses. The kinship between Afro-Asiatic (Hamito-Semitic) languages was widely discussed ever since the middle of the 19th century, and Adolf Erman in the beginning of the 20th century did not doubt much about the relatedness of Egyptian and Semitic. The Afro-Asiatic hypothesis was formulated in much detail by Marcel Cohen, Greenberg's contemporary, in 1947 [Cohen 1947].

The idea of kinship between the languages of West, Central and South Africa, known as Niger-Congo since Greenberg, was actually proposed a century before. In an introduction to his collection of glossaries of languages of Mozambique, Wilhelm Bleek [1856] wrote: "The languages of these vocabularies all belong to that great family which, with the exception of the Hottentot dialects, includes the whole of South Africa, and most of the tongues of Western Africa; certainly the Otsi, or Ashantee, the Bullom, and the Timneh of Sierra Leone. The Gōr family, which includes the wide-spread Fulah, the Accra, and the Wolof, may be considered as related to these; as may also the Ukuafi, spoken near the source of the White Nile, and the Tumale in Darfur."

It should be noted here that Bleek mentions Fula among those related languages of Sub-Saharan Africa, another discovery attributed to Greenberg. The later "Hamitic Myth" actively promoted by the giant authority of Karl Meinhof and Alice Werner in the early 20th century overshadowed Bleek's hypothesis a little, but Diedrich Westermann actually said pretty much the same as Bleek in his [1927] paper on Sudanic languages.

The same must be said about the Khoisan languages which, because of their distinctive phonological features (click consonants) and even more distinctive anthropological traits of their speakers have been identified and treated as a whole long before Greenberg.

But what Greenberg actually did was to gather all these early scholarship attempts at classification and to construct a logical and uncontroversial system out of them. Before him, linguists were mostly guessing; Joseph Greenberg postulated these guesses into an elegant theory which summarized early achievements and could encompass all languages of the huge continent. Moreover, he was the first who tried to prove these early guesses with a mass of lexical data, even though this method of his has always been regarded as his weakest point.

2. Greenberg's Method

The method of mass or multilateral comparison used by Greenberg in his 1963 work (as well as many papers afterwards) has been increasingly criticized by the community of comparative linguists. The main thesis usually proposed to disqualify this method states that Greenberg projected too scant data to conclusions that were too far-reaching.

Indeed, in his 1963 research on Niger-Congo we can find 49 lexical items taken from 186 languages (out of the 1,500 which are currently attributed to Niger-Congo). The Niger-Kordofanian addendum to the paper adds to this 52 correspondences between Niger-Congo and Kordofanian. The lexemes used mostly belong to the basic lexicon but not limited to the Swadesh list or any other selection. No explanation is provided on the principles of selecting either languages or lexemes, so they might look random to those accustomed to a strict methodology. Taking into account the huge number of languages in the macrofamily, one may imagine that it would be possible to find a cognate for almost any lexical item which will be a pure coincidence in fact. No attempt was made to work out a system of regular phonetic correspondences between the families within Niger-Congo or to reconstruct the proto-language phonological system. Apart from a brief analysis of noun class markers across Niger-Congo, no morphology was analyzed. So, one may ask, how could Greenberg prove anything with such an imperfect and incomplete method?

However, the author of Niger-Congo actually did not intend to prove anything. He always regarded his idea as a "first proposal" [1977], not a final solution to the genetic classification of the languages of Africa. It would be completely wrong to view Greenberg's paper as the proof of the classification. He did not aim at creating an ultimate systematic reconstruction of Proto-Niger-Congo, nor did he plan to establish a system of phonetic correspondences or a morphological database. This goal could probably not be achieved at that time with the volume of data accessible to date, when most African languages were known by just their name or at most a small glossary of inaccurately recorded lexemes.

Later Greenberg always emphasized that his method of multilateral comparison can only be treated as a proposal, a dotted line which was still to be verified or modified by means of the strict comparative method, of which Greenberg remained a passionate supporter through all his life.

Consequently, it would be absolutely inappropriate to try to prove Greenberg's hypothesis without the utilization of the comparative method. Both mass comparison and any other types of lexicostatistics widely used in African linguistic literature, will inevitably suffer from a subjective approach. At the same time, the typological approach, also quite popular in African linguistics in the West in the 20th century, may be characterized by exaggerating the role of typological characteristics of languages in defining their genesis. Speaking about Niger-Congo, anyone would now agree that the presence or absence of the system of noun classes cannot be regarded as a valid proof of kinship. The opinion that languages using noun classes should be included in Niger-Congo and those lacking them should be excluded from it, is still seen sometimes in linguistic papers, but is surely wrong. Nominal classification is a structural phenomenon and can appear both as inherited and as areally spread.

No other method may be used for confirming Greenberg's African hypothesis (or suggesting another one in this field) but the comparative method. But was it ever done so in practice?

3. Greenberg's Legacy

In recent decades, "splitters" seem to overwhelm "lumpers" in African linguistics. Their claim is that there is not enough proof that the mid-level families of African languages are related, and that at least some of the larger groupings should be reconsidered as areal or areo-typological units rather than genetic ones. Splitters continue arguing that Greenberg's language map of Africa was not confirmed by any strict scientific method despite all the efforts made in the past 50 years.

The truth here is that almost no attempts in fact have been made to verify Greenberg's Niger-Congo hypothesis. This might seem strange but the path laid by Joseph Greenberg to Proto-Niger-Congo was not followed by much research. Most scholars have focused on individual families or groups, and classifications as well as reconstructions were made on lower levels. Compared with the volume of literature on Atlantic or Mande languages, the list of papers considering the aspects of Niger-Congo reconstruction *per se* is quite scarce. Apart from efforts of Hans Mukarovsky [1976-1977] and John Stewart [2002 etc.] who proposed their pilot versions of reconstructing the proto-language, not quite in line with Greenberg's ideas, not much has been done in this regard.

This is true both for lexicon and grammar, let alone phonology. As for today, most objective problems that Greenberg must have faced regarding the amount and quality of comparanda for comparative analysis have been overcome. Hundreds of new language descriptions have appeared in free access, and a dozen new ones appear annually as theses, monographs or articles. Numerous dictionaries follow grammars. The level of their accuracy has increased greatly, since they all now mark tones (a rare advantage in the 60s) and in general reflect more or less adequately the complicated phonetics of African tongues. For a great number of languages, we can compare between two or more descriptions. Sure, the number of languages left undescribed is still big, and new discoveries are still made (the Russian Linguistic Expedition to West Africa alone has proudly discovered two new Niger-Congo languages in the past four years), but the amount of data is quite sufficient to make a decent lexical database including at least Swadesh 100/200-item lists. This is still to be accomplished.

Until the lexicon is treated systematically, no advance can be made in reconstructing phonetics. It is fully understood that the reconstruction of an uncontroversial phonetic system for a proto-language of over 10,000 years is a challenge. But other postulated macrofamilies of the world, including Afro-Asiatic and Nostratic, can already boast huge progress in this field. For Niger-Congo, no decent proposal has been made for the reconstruction of its phonology which would include lexical data with correspondences.

Grammatical reconstruction is another big task, which may seem the most important from the point of view of splitters of the 21st century. As Campbell & Poser [2008] rightly note, morphological reconstruction is the most convincing evidence for any deep language reconstruction, given the wide spectrum of various speculations a lexical reconstruction may provoke. The Afro-Asiatic hypothesis was mainly based on grammatical comparisons, and the Indo-European one started with them. In Niger-Congo linguistics, the only aspect which was considered by linguists in more or less detail, is the system of noun class marking. More research in morphology should generate a plausible reconstruction of Proto-Niger-Congo which will turn the largest macrofamily of languages in the world from a phantom into a reality.

This is how we can save Greenberg's legacy.

4. Reconstructing Niger-Congo person marking

The following briefly summarizes the results of the comparative analysis of Niger-Congo person marking systems published in [Babaev 2013] in Russian. The objective of the research was to perform a consistent step-by-step analysis of person marking in as many languages of the macrofamily as possible, and to suggest a systematic and uncontroversial reconstruction of what the proto-language system of person marking could look like both in terms of forms and meanings. Such a task was dealt with for the first time.

Data from over 650 Niger-Congo languages were used in the research, with all the major families and groups of languages included. This allowed the creation of the biggest database on personal markers in Africa and the provision of a necessary level of reliability for the conclusions made. All the data was presented according to a unified structure of four syntactic series of person markers: subject markers, direct / indirect object markers, possessive markers, and independent pronouns widespread in Niger-Congo languages to mark focus or topic or to serve in nominal clauses. Person marking was analyzed by family, in accordance with the currently recognized genetic tree of the macrofamily, starting from Benue-Congo and then up to Atlantic, Mande, Dogon and Kordofanian. For each family, a reconstruction was made on the basis of lower-level reconstructions of person marking systems for groups and subgroups where necessary. This means, e.g., that the Proto-Benue-Congo reconstruction was not suggested before Proto-Bantu, Proto-Cross River, Proto-Central Nigerian, Proto-Edoid and other reconstructions were elaborated. This cautious, detailed and scrupulous approach allowed one to solidify the use of the comparative method and increase the level of reliability of the results received.

As the main conclusion of the research, Greenberg's idea of Niger-Congo should be confirmed. The core families of Niger-Congo (the so-called Volta-Congo languages including Benue-Congo and Kwa, Kru, Gur, Adamawa, Ubangi, and Atlantic) show distinct genetic relationship in person marking, not only in the shape of individual morphemes but also in paradigms which is a much more stronger argument for kinship. The Dogon and Kordofanian data also confirm their status as Niger-Congo, even though more distant from the core.

It appeared however that the same cannot be stated for sure for Ijoid and Mande, the two families whose Niger-Congo affinity may not be reliably established through the system of person marking. Both families must have either been more distant relatives of Niger-Congo, departing from the proto-language community very long ago, or (in the case of Ijoid) may not be non-Niger-Congo at all, having adopted some of the Niger-Congo language features due to lengthy and intensive language contact. This conclusion, agreeing with some earlier research (see [Dimmendaal 2011]), should however be reconfirmed by further analysis.

At the same time, person marking systems of other languages of Central and West Africa were also included in the analysis in order to demonstrate a proof by contradiction. Person markers from Chadic, East and Central Sudanic, Songhai, and Kadugli-Krongo languages, all adjacent to the Niger-Congo-speaking area, show drastic differences in form and meaning from those of any Niger-Congo branch. Contrary to the statements of some Niger-Congo skeptics, this macrofamily has its distinct borders and may not be projected to the other families of African languages.

Some interesting conclusions on the internal classification of Niger-Congo can be made from the research. The boundary between Benue-Congo and Kwa appeared to be almost non-existent, and Kwa should probably be treated as a collection of branches of a single node (Benue-Kwa) rather than a single node. Several groups, formerly regarded as peripheral Gur languages, including Senufo, seem not to belong to Gur but rather should form separate branches of Volta-Congo. The same may be true for Ubangi and especially Adamawa, where several subgroups (e.g., Yungur) do not show any Adamawa affiliation whatsoever.

All these conclusions, however, must be taken into account only as hypothetical, since the system of person marking is not the only and ultimate marker of language kinship, even though it can provide a strong argument on the issue. Further research in morphological systems of Niger-Congo are now essential to provide additional evidence for the validity of the macrofamily, including the research in noun class marking, in verb extensions, verbal auxiliaries, numerals and other paradigmatic systems so important for the proto-language reconstruction. The more detailed comparative research we present to African linguistics, the clearer will be the picture that Joseph Greenberg sketched for us fifty years ago.

Literature

Babaev 2013 - Бабаев, К.В. 2013. Нигеро-конголезский праязык: личные местоимения. М., ЯСК. [*Niger-Congo protolanguage: personal pronouns.* Moscow: "Languages of Slavic Culture."]

Bleek, W. 1856. *The languages of Mosambique: vocabularies of the dialects of Lourenzo Marques, Inhambane, Sofala, Tette, Sena, Quellimane, Mosambique, Cape Delgado, Anjoane, the Maravi, Mudasu etc.* London: Harrison & Sons.

Campbell, L. & W. Poser. 2008. *Language Classification: History and Method.* Cambridge: Cambridge University Press.

Dimmendaal, G. 2011. *Historical linguistics and the comparative study of African languages.* Amsterdam – Philadelphia: John Benjamins.

Greenberg, J. 1963. *The Languages of Africa.* The Hague: Mouton; IJAL 29.1, Bloomington: Indiana University Press.

Greenberg, J. 1977. "Niger-Congo noun class markers: prefixes, suffixes, both or neither." // Mould, M. & T.J. Hinnebusch (eds). *Proceedings of the 8th conference on African linguistics, Suppl. 7 to Studies in African linguistics.* Los Angeles: African Studies Center & Dept. of Linguistics, University of California. Pp. 94-104.

Mukarovsky, H. 1976-1977. *A Study of Western Nigritic.* Vols 1-2. Vienna: Institut für Aegiptologie und Afrikanistik.

Stewart, J. 2002. "The potential of Proto-Potou-Akanic-Bantu as a pilot Proto-Niger-Congo, and the reconstructions updated." *Journal of African Languages and Linguistics* 23: 197-224.

Westermann, D. 1927. *Die westlichen Sudansprachen und ihre Beziehungen zum Bantu.* Berlin: de Gruyter.

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

Joseph Harold Greenberg: A Tribute and an Appraisal[1]

Harold C. Fleming
Boston University

I will eschew a standard obituary format because, for those interested in the biographical details of his life, there are, have been, and will be ample sources of these published. Above all we had Paul Newman's long biographical interview with Greenberg from *Current Anthropology*, paraphrased in *Mother Tongue*: The Newsletter (1991). More recently, Nicholas Wade had a long obituary in the *New York Times* (May 15, 2001) which did an excellent job.[2] And some aspects of biography will be included herein.

THE TRIBUTE

When an important man of science leaves us, we think of how much we benefitted from his work, how much he stimulated our work, and how things will be now that he is no longer working among us. Perhaps the most important first question is just how important was this scholar anyway? Apparently one would not be alone to say that Joe was a great scientist, easily one of the three or four most influential linguists of the 20th century and easily the top man in the genetic taxonomy of languages who ever lived.

His outstanding work on typology is not included here because I paid it little attention; yet it is clearly another kind of comparative method, one more familiar to anthropology and the other social sciences. So Greenberg excelled at two kinds of comparative strategies, the one genetic and historical or simply diachronic, and the other synchronic or achronic, closer to 'the physics model' or what most philosophers of science think of as the scientific method. One seeks to find the conditions under which certain kinds of phenomena occur and thus establish general laws for the occurrence of these phenomena, and test them—ideally through experiments. Since being well trained in this comparative method, as used in kinship studies and ethnographic surveys, by G.P.

---

[1] Reprinted from *Mother Tongue* VI (2001).

[2] Except that Wade and his informant, Paul Newman, forgot the large contribution that was Greenberg's Indo-Pacific hypothesis. Since he had already (1954) commented in depth on Southeast Asia, his regrets were possibly due to not returning and settling the Austric question. Conservative Australian and British linguists have largely rejected the Indo-Pacific hypothesis which argues for its probably being right. They acted the same way in Africa. [The reference to Paul Newman's interview is *Current Anthropology,* vol. 32, no. 4, August-October 1991, pp. 453-467. Excerpts were included in *Mother Tongue* (Newsletter) 15, December 1991. Ed.]

Murdock, the difference between the synchronic methods of ethnology and the diachronic methods of genetic linguistics is very clear to me. However, this difference is often not understood by proponents of the two strategies, such as physicists and historical linguists. There has been confusion in archeology about this difference too.

One test or confirmation of the dual skills that Greenberg possessed is given by his election to the National Academy of Sciences, the highest scientific honor one can get short of the Nobel Prize in Prehistory, which has never been awarded. He was also elected to the American Academy of Arts and Sciences. Indeed, when once I solicited his résumé, his honors from various universities and scholarly groups took up as many pages as would suffice for an ordinary scholar's entire résumé!

Two more anecdotal pieces of evidence for the high regard that so many scientists and scholars bestowed on him are offered here. Once Frank LeBar of Yale (Human Relations Area Files) discussed the classification of Miao and Yao of Southeast Asia with Paul Benedict. One key point of their discussion was whether Greenberg had changed his mind or not on that subject (which he had), because anthropologists were completely dependent on Greenberg's opinion—such was their respect for his genetic hypotheses plus his great prestige. A second example came from my own department (BU). When once I told a colleague (Anthony Leeds) of my exciting discovery of some new aspect of Afrasian taxonomy, he replied: "That sounds good. Did you check it out with Greenberg yet?"

At least until 1987 Greenberg alone probably was regarded by more social scientists as the world authority on genetic classification than any other one, or two, or three scholars anywhere. After 1987 the furious opposition to his Amerind hypothesis by Americanist linguists and some Indo-Europeanists changed much of the public scholarly perception of his work. His African work remained virtually impregnable; it had been tested for almost 40 years and had held up. His Indo-Pacific hypothesis was increasingly ignored, while the Amerind effort became a battleground. While Ruhlen defended Amerind mightily, and *Mother Tongue* devoted much of its effort to Greenberg's defense, most Americanists turned away from Amerind to devote themselves full time to nitpicking. Greenberg's methods were scorned and he was thought of as a scholar whose best days were far behind him. Very great social pressure was exerted upon linguists to conform to the critiques of Greenberg; even his own students were frightened into silence. As Joe[3] told me himself several times, the Americanists, and increasingly 'the linguists', had embraced a new paradigm, albeit a mistaken one, and they disappointed[4] him a lot.

---

[3] Greenberg has always been called 'Joe' among anthropologists. That is a tribute to his warm and modest behavior and the obvious affection with which he was regarded. We will stick with Joe from now on.

[4] 'Disappoint' is used deliberately. Joe was extraordinarily rational, non-confrontational, and mild-

Yet, despite his advanced age (ages 75 to 85), he devoted the 1990s to work on the Eurasiatic hypothesis, finishing a first volume on comparative morphology a few years ago and then completing its matching lexicon just before he died. When doctors told him that he had incurable cancer (in mid-winter) and would soon die, he shrugged and kept on working until he and Merritt Ruhlen had finished the lexicon. Nicholas Wade reports that Paul Newman asked Joe shortly before he died what his greatest regret was. Joe said it was his failure to finish up Southeast Asia! Merritt and I saw him not long before he slipped into his final coma, making us the last scholars to talk with him. Yet even then his mind was clear and rational. Thus I mentioned that Gilyak had a word, roughly /irf/, meaning fox or jackal, and that it was found in various places in western Eurasia and north Africa. (This was in a context of discussing Karl Bouda's work on Gilyak.) Joe said it was really something like /iRf/ because the *r*'s were like German or French *r*'s and that it was part of an etymology in the Eurasiatic lexicon. What a memory! What a scholar! And what a shame that his vast and unique knowledge of human languages had to leave us, could not be electronically stored, and that such a great scientist had to die under a cloud of misguided criticism!

We can ask how anyone on their deathbed could be thinking about Eurasiatic or even Gilyak etymologies. The answer I propose is that this was what he was good at, this was what he loved, and indeed, this was his life. To his core Joe was an exemplary old-fashioned or traditional historical linguist. During our farewell visit (*Abscheidsfeier*), he said that he had begun thinking on his own about language when he was 12 years old. Nobody told him about it but he noticed phonetic patterns in English, his own and others, and puzzled it until getting a conclusion. Self-taught at twelve.

Who then trained Joe in linguistics? What school did he represent? A well-kept secret perhaps, but the answer is—NOBODY. He was trained in cultural anthropology, did his field work in Nigeria on acculturation to Islam, and wrote his dissertation on that topic. He greatly admired Edward Sapir, whom he resembled cognitively, but he never studied with Sapir. He read Sapir and the great books on Indo-European and a great many works of 19th century German scholarship; from his youth he had read grammars for pleasure and remembered them. It helped that his mother spoke German, which thus was far easier for him than for the rest of us Americans for whom scientific German was a chore.

Three years ago at a conference in Baltimore one of our Russian colleagues in an excess of Neo-Grammarian zeal proclaimed that "Greenberg is an amateur!" (The same for Ehret and probably me, when I left the room). The accuser, Militariev, was the Russian

---

mannered. As I told him just before he died, "I am the emotional one, you the rational; so I am going to tell you how I *feel*." Once, when Joe was being attacked by British linguists and Semiticists— as usual—, Dan McCall asked him why he didn't fight back. Joe replied that evidence would decide the matter in due course.

who had first contacted me in Moscow in 1986, thus a co-founder of ASLIP was he. At least four times at that conference Militariev contrasted 'amateur' with 'professional'. Well, a professional was guided by Neo-Grammarian principles, while an amateur was a lower form of life. Despite my irritation at his arrogance, I later slowly realized that there was some truth lurking in his contemptuous remarks. In a sense Joe was an amateur, not having been trained like an apprentice by a master linguist and not having had his 'mistakes' (deviations) corrected repeatedly. This is not to say he never had a course in linguistics, never talked to a senior linguist, and never was told how linguists do their thing. He did have a little of that, but overwhelmingly he was self-taught—by reading voraciously and by thinking. Or, by using the common sense for which he was famous. And oddly enough the other two amateurs, Ehret and me, were very much the same; Chris was trained to be a historian but took a few courses in linguistics. I had two semesters with a trained linguist, Lounsbury, but that was all. (The course was required for all first year graduate students at Yale.) I was trained to be an ethnologist. Likewise some of our most productive or creative long rangers—Bomhard, Bengtson, Hayes, Whitehouse—are amateurs in the Greenbergian sense.

What was most astounding about the life work of Greenberg was not so much the ground he covered—which was immense—but the singularity5 of his contribution. His ventures or hypotheses extended from 1948 (the first African articles in *Southwestern Journal of Anthropology [SWJA]*) to 2001 (the final lexicon of Eurasiatic); fifty-three years' worth of scientific creativity, i.e., hypothesis formation. In a moment we will list the noteworthy points where he extended our knowledge of linguistic prehistory in fruitful and reliable ways. For now, however, it is appropriate to ask: during those 53 years when Joe's cognitive fingers probed into prehistory, where were the professionals? What were they doing? What hypotheses about our common past did we get from them? Precious little, bloody little, damned little; what you call it depends on your dialect. Granted there were some active scholars, but we are not obliged to name each one because none of their contributions were both as extensive and as reliable as Joe's. There is always what linguists generally call the 'lunatic fringe' where individuals will propose daring hypotheses that usually fail to stand elementary testing or just get ignored. Mostly Europeans, their names include such as Mukarovsky, Bouda, and Pinnow, some of whose ventures are just now getting accepted.[6]

Two major exceptions to these conclusions about professionals exist. One is the

---

5 This notion is borrowed from Nicholas Wade's obituary wherein he referred to Joe as a "singular linguist."

6 For example, Pinnow's old contention that Haida belongs to Na-Dene as a coordinate was accepted by Greenberg, but only this year by some Americanists, and not yet by the Russians.

work of Morris Swadesh in North America and the other the work of Illich-Svitych in Russia, with his colleagues Dolgopolsky and Dybo, and their students. Swadesh brought the concept of linguistic dating to fruition, although the resistance to his glottochronology was even fiercer than the attacks on Greenberg. Swadesh also attempted the ultimate taxonomy of the world, as Trombetti had done, but his efforts were not reliable and his mass of etymologies apparently never got published. Swadesh was an anthropologist but also a true professional linguist who did fine work on Amerind languages and some theoretical linguistic concepts (e.g., the phoneme). He and Joe worked together for a while in their youth but clearly were not good friends.

In Moscow the original daring work on the 'lunatic fringe' by Pedersen on the relatives of Indo-European was checked, accepted and expanded by a small and highly creative group of young Russian professionals. They and their students produced Nostratic and later Dene-Caucasic, both bold and exciting ventures. They backed up their boldness by attempts at strict phonological controls, elaborate reconstructions, but massive compilations of data. More than anyone else, even including Greenberg, they broke the stranglehold of Indo-European exclusivity, the unacceptable notion that Indo-European had relatives, especially in the Mongoloid realms of the east. The resistance to this Russian work was much softer than that to Greenberg, but final acceptance has not yet arrived. Joe's work might help to push the matter over the threshold. Dare I say that the Indo-Europeanists seem more tolerant or more rational than their colleagues in the Americanist 'mainstream'? Sure, cultural anthropologists can say that sort of thing. Why not? 'Tis true.

Let us sum up Joe's singularity, why his contributions just dwarfed anyone else's and why he accomplished more than hundreds of American professional linguists *combined* in his 53 years of hard work.

AFRICA

Between 1948 and 1963 he reviewed the literature on African languages and taxonomy, fought free of widespread European racial superiority assumptions, broke the bond between physical type and language genetics, and put some 1,500 languages into four large taxa where almost all have stayed ever since. Despite the belief among some woefully ill-informed American linguists that African languages are close to each other, like Bantu ones are, there are huge differences in phonology, morphology and vocabulary. In all of the phyla lexical retentions on a Swadesh list get down to 1%, for example; just in Afrasian (formerly Hamito-Semitic) between Berber and Omotic languages or Berber and South Cushitic languages we reach that low percentage. Or in Niger-Congo between West Atlantic (e.g., Peul or Fulani) and Kordofanian. Or between North Khoisan and South. And so forth.[7] Within each of major phyla (families) relationships often get quite remote. In some cases the remoteness leads to the relationships being questioned. For example, Songhai within Nilo-Saharan, Omotic within Afrasian, Hadza and/or Sandawe within Khoisan: each has been challenged—ultimately unsuccessfully.

SOUTH & SOUTHEAST ASIA

In 1954 *Anthropology Today*[8] published a state of the art book of theory, involving the four sub-fields, with articles written by leading scientists in special fields. In the book Joe published a theoretical piece on new methods in historical linguistics. But he included in that a survey of some areas with unsettled questions. Having looked over the literature and much of the data, he ventured opinions that *faute de mieux* added up to a taxonomy of most of the world. In South Asia he agreed with traditional phyla such as Indic, Dravidian, Munda, and Tibeto-Burman but also stipulated that Nahali (Nehari) was distinct. He supported Paul Benedict's separation of Thai-Kadai from Sinitic and Pater Schmidt's creation of a large phylum called Austric. Joe missed Kusunda in the Himalayas, which was easy to do since it was buried in masses of Tibeto-Burman material in Grierson's *Linguistic Survey of India*. Almost everyone else missed it too.

THE SOUTHWEST PACIFIC & THE INDO-PACIFIC HYPOTHESIS

While not venturing much outside of Malayo-Polynesian (Austronesian) and the mainland phyla, Greenberg was clearly stimulated by the problems of Melanesia and Papua, and later Australia and Tasmania. That area which we now suspect contained the first emigrants from the *Homo sapiens* homeland in Africa has linguistic and cultural diversity to match that of Africa or the New World. And it is OLD! Some evidence is found in the not-quite resolved archeological dates for Australia for 40,000 to 60,000 or more. But other and in some ways more interesting archeological dates come from insular Melanesia, where dates of 38,000 more or less are found. That settlement had to be sea-borne and most probably came from Papua, long before anyone would seriously propose that Austronesian sailors were involved.

Joe took twenty years to examine the hundreds of languages that physical anthropologists usually called the 'NAN' peoples, the non-Austronesian peoples of the western Pacific and Indian Ocean. Roughly the region from the Andaman Islands to Fiji, and from Tasmania to the Admiralty Islands, was the domain of his inquiry. Naturalists have observed of flora and fauna that northern climes have fewer species but larger populations while tropical climes have more species but smaller populations. That observation aptly portrays the human language situation, especially in Oceania, Africa and Latin America. Add to that the older biological conclusion that modern man is a tropical animal who has adapted culturally to northern climes.

---

[7] Sergei Starostin has maintained that two languages having less that 5% on a Swadesh list should not be put in the same family. That is a serious confusion of mathematical probability thinking and the bases of linguistic classification which are not limited to Swadesh list vocabulary and include grammar, etc.

[8] *Anthropology Today* was edited by A.L. Kroeber, at that time arguably the most influential anthropologist in the USA. Anyone studying for their comprehensive exams in anthropology felt obliged to read it.

By Ruhlen's count in his *A Guide to the World's Languages* (1991 edition) there are 731 cognate languages that are neither NAN nor Australian in this Oceanic realm. Joe proposed calling them Indo-Pacific, after his customary use of geographical terms to label linguistic taxa; he finished his classification in 1971.[9] Perhaps the biggest surprise of Indo-Pacific was its inclusion of Andamanese and Tasmanian, as far apart geographically as Berber of Morocco and !Kung of the Kalahari. A second surprise was separating Tasmanian from the Australian phylum a short distance away on the mainland, yet joining it to Papua a whole continent apart. A third point, although not so surprising, was Joe's refusal to link the Australian and Indo-Pacific phyla together. Had he done so he would have proposed the oldest linguistic taxon on earth, remembering those archeological dates above. Its African equivalent would be to link Afrasian and Khoisan or Nilo-Saharan with either of them.

It is noteworthy that Greenberg observed limits, i.e., he has never formally proposed a taxon older than his African foursome or Indo-Pacific. But he has suggested in a number of places that there were probably older taxa around, for example, Afrasian and Niger-Congo, Khoisan and Afrasian, Amerind and Eurasiatic, etc. He simply lacked the time and energy to try to establish them. And perhaps, considering the furor most of his proposals elicited, he was just tired of being yelled at!

Thirty years after Joe's Indo-Pacific proposal one cannot say that it has been accepted. No doubt some scholars, probably mostly anthropologists, quietly believe it is a viable hypothesis. No doubt some others consider it 'unproven' or foolish or the like. They are likely to be Australian or British, but they are usually quiet about it, not abusive. Mostly Indo-Pacific has been ignored, nearly to death. One major hope is offered by Paul Whitehouse (London) who is embarking on a grand review of the numerous new data on NAN languages plus the old etymologies Joe proposed. Since he has already convinced himself that Joe was right—that Indo-Pacific is viable— the future looks brighter for this somnolent piece of prehistory.

---

[9] Cf. J.H. Greenberg, 1971, "The Indo-Pacific Hypothesis." *Current Trends in Linguistics*, volume 8. Ruhlen reports manuscripts written in 1958 and 1960 but unpublished.

THE NEW WORLD AND THE AMERIND HYPOTHESIS

In anthropological linguistics of mid-20th century social science a large part of scientific activity was focused on the Americas. With the work of Sapir, Kroeber, and indeed most of the Boasian school of anthropology being influential, and the ready availability of local informants, the work most people heard about concerned Native America. The only other major focus was on Indo-European; that existed primarily outside of anthropology and had a considerable tendency to play by its own rules. There were few departments of linguistics, and indeed perhaps only twenty of anthropology.[10]

After finishing his training at Northwestern University in cultural anthropology under the dynamic Melville Herskovits, Joe went to Africa to do his field work in cultural anthropology (ethnology). When he returned and got employment at Columbia University, he was already familiar with the powerful Boasian milieu from his college days. Yale was not far away, where Edward Sapir had taught until 1939, and from which his student, Morris Swadesh, came to New York to teach at CCNY. Joe and Morris came into close contact and undoubtedly influenced each other. There are anecdotes that circulate among anthropologists about the seminal Greenberg-Swadesh interaction, but suffice it to say that the languages involved were native American. The conclusion has to be that Greenberg began work on Amerind before his African classification was finished.[11] Or he was working on Amerind languages before many of his Americanist critics.

By 1960 Joe had reduced the diversity of Central and South America to far fewer phyla or families than the prevailing picture of scores of independent families in Latin America. He gave another paper on classification in 1979 and another in 1981. Six years later he published his *Language in the Americas,* which gave his full hypothesis. Later on, some additions and changes to internal taxonomy were ventured by Merritt Ruhlen, but

---

[10] In the post-World War II atmosphere, especially with massive governmental stipends to students or the GI Bill of Rights, the number of anthropology departments increased rapidly. By 1970, when this trend was aborted fairly abruptly, anthropology departments numbered slightly more than 100. Linguistics departments picked up steam later than anthropology but also were abruptly cut off by the early 1970s, which saw linguists increasingly seeking employment in anthropology departments. The new departments had produced too many PhDs and the glut contributed heavily to the aborted growth curves.

[11] I am indebted to Daniel McCall for conversations over many years about those famous Boasian days in New York. Dan was also one of Greenberg's first graduate students at Columbia. I would surmise that Greenberg's interest in African linguistics was triggered by his field experience in Nigeria where he found that Hausa of Chadic was not clearly grouped with many languages that were obviously related to it. Thus began the emphasis on Chadic that dominated his chapters on Afrasian (his Afroasiatic) in his first articles in *SWJA* in 1948.

Joe's final effort on Amerind appears to have been in 1987.

Why did it take nearly 40 years to fully classify the 583 Amerind languages into one phylum while the much greater number of African languages were classified much more rapidly into four phyla? Actually, the full and final African classification took 15 years, waiting in the last ten years for the deeper linkage of Niger-Congo to Kordofanian and the gathering together of several independent families into the Nilo-Saharan phylum or super-phylum. There seem to be five primary reasons for the greater amounts of time required for Amerind.

1) In Africa the 'transitivity principle' was easier to apply, because there were broad stretches of closely related languages adjacent to areas with more distantly related languages in turn adjacent to other related languages. To take the extreme example of Bantu we find hundreds of closely related languages spread over an area as big as the USA west of the Mississippi. That joined to a so-called Semi-Bantu or Bantoid in a much smaller area; that in turn to other groups in southern Nigeria; and so forth. The basic principle is a matter of logic. If A is related to B and B is related to C, then A is related to C. That principle fit most of Niger-Congo, Afrasian, and southern Khoisan. It did not do so well in Nilo-Saharan where languages such as Songhai, Saharan, Fur, Kunama, and Nyangeya were not only physically distant from each other but also not at all close linguistically. On the other hand it is not common in the Americas to find phenomena like Bantu or Arabic with their wide distributions.

2) In Africa the ground had been prepared by lumpers; in America by splitters. Not a few 19th century and early 20th century scholars, such as Koelle, Johnston, Cohen, Westermann, and Meinhof, *et al.,* had been inclined to assemble great globs of data or to make sweeping classifications based on a few typological traits. In a sense the job was to correct their errors, account for the ones they missed, and put it all together. For most of the African lumpers their gross error rate was not so excessive. Granted, they made serious mistakes, but most of what they linked together was usually genetically true. Most of Afrasian was already laid out, Bantu and Khoisan were already in the literature, much of Niger-Congo under the name of Sudanic was in place, and parts of Nilo-Saharan. In more modern times Africa produced a generation of hyper-splitters of British origin whose distrust of hypotheses of relationship could match the amazing splitters of South America. Had Africa been left to their tender mercies it would resemble Latin America with scores of independent phyla. The received literature in the New World was much like it is now: genetic groups are small and numerous. However, Sapir and some others had gone far to modify that condition. But controversially.

3) In the New World many anthropologists found 'culture areas' in which much of the culture was widely shared among neighboring peoples. But Amerind language diversity is extraordinary in that it is frequently the case that few languages have close relatives in

their own area. It is as if France, for example, contained French, German, Swedish, Russian, Hindi, and Armenian. There must have been a history or prehistory of mobility that produced such local diversity. Take the strange case of Algic with California branches separated from midwestern branches and separated from eastern branches, each of whom found themselves with seemingly unrelated neighbors, like Iroquois in the east or Penutian in the west. Or follow the distribution of the various large branches of Central or Southern Amerind in the Amazon basin. They resist analysis into homelands or geographical foci. It all looks more like a scattergram or scatter shot or a work of modern high-tone art. Actually, a major modern city like New York would be something like this if each of the entering ethnic groups had kept their language and lived in their own communities or 'tribes'. Africa is much like this in some areas but also has vast areas with little diversity. Only the Arctic in North America is anything like most of northern Africa.

4) Scholarly work and/or sources were more numerous and older in the New World than in Africa. Despite the great antiquity of two northern branches of Afrasian (Semitic and Egyptian) and some lesser antiquity for Ethiopic and Arabic of Islam, most African languages were described in the 19th and 20th centuries. A few on the western and southern coasts were contacted by Portuguese and Dutch explorers in the 17th century, but the records are not very full. On the other hand a large part of the Americas was described as early as the 16th century by Portuguese, Spanish, French, and English explorers and colonists. More to the point, anthropology and linguistics were growing up in the 19th century in Europe and North America. Just as the literature of Indo-European is a much more vast enterprise than any other of its ilk, so the Americanist literature was much larger and more sophisticated than that of Africa, which basically consisted of a few Europeans writing about African languages. So there is probably more data and grammatical analysis to read per language and a lot more reconstruction per group of languages.[12]

5) Greenberg's critics were more numerous and better organized in the Americas than they had been in Africa. While Greenberg did have severe critics among Semiticists, such as Wolf Leslau, most of his opponents were European. While British linguists were overwhelmingly hostile until recent times, the best European linguists were in France, Italy, and Germany; they were much less hostile and many were converted early on. In the Americas, on the other hand, Joe's critics were given years in which to decide what to do about his classifications (see above for 1960). They were a much more compact group and represented the victory of Indo-European thinking over the old Americanist ways. They were in effect organized by a series of introductory texts in linguistics that stressed methods, rigor, precision, and something like a Neo-Grammarian position. Moreover, since

---

[12] Greenberg was criticized by numerous Americanist scholars for 'mistakes' (usually phonetic imprecision or erroneous morphological segmentations in grammar) or failures to use 'modern' reconstructions, i.e., their own work.

the Chomskyite revolution had swept American linguistics after 1957, many historical linguists probably felt threatened by the changes proposed by (1st) Swadesh, (2nd) Chomsky, and (3rd) Greenberg. Swadesh was practically run out of the country because of glottochronology, although many say the reason was his being a Communist. Chomsky himself told me years ago that he was appalled by the hostility he received from linguists. When interviewed by Nicholas Wade after Greenberg's death, L.L. Cavalli-Sforza (of biogenetics fame) is quoted as saying that Joe's critics were cruel, probably because they were jealous of his successes.

As a final note, Greenberg's Americanist critics were successful in one thing. They apparently convinced their colleagues in the rest of linguistics that Joe's work was under par and mistaken and in fact anathema. So today most of American linguistics is opposed to the Amerind hypothesis and the methods by which it was created. All of these developments were apparent to me in the late 1980s. I tried to warn Joe indirectly via an opinion in *The Atlantic Monthly,* but their editor cut the warning out for reasons of space.

The Amerind hypothesis, *qua* hypothesis, was a sweeping vision of the entire New World, since it was grounded in the notion that there were two other phyla present. By proposing that Eskimoan and Na-Dene were independent of Amerind, he contributed greatly to prehistory. With its representatives stretching all the way south to Cape Horn and eastward all the way to Labrador (Beothuk), Amerind was the obvious choice for first human occupancy of North and South America. And the great internal diversity of that taxon argued separately for a considerable antiquity of Amerind in the New World. Greenberg had decided independently that the age of Amerind in the New World was to be correlated with the archeological dates of first human entry. In association with Christy Turner (archeology) and Stephen Zegura (physical anthropology), he agreed to 12,000 BP as the likely date for that entry. Since more recent archeological research has increasingly challenged that date, the so-called Clovis horizon, Joe has not changed it. Although Ruhlen continues vigorously to defend the date, on Joe's behalf, I think it is a basic error on their part and their conclusion is being undone by current archeology.

EURASIA AND EURASIATIC

While Africa is huge, the continent of Asia is even bigger. Combined with Europe, it becomes Eurasia, the largest of all the great land masses on earth. From a geographical standpoint most of Europe is a large peninsula of western Eurasia, with Arabia and India the same to the south. On the southeast the Malay Peninsula almost joins the insular world of Austronesia or Sundaland, which was cut off only when the Ice Age ended.

We have considered the southern parts of Eurasia above. The northern and western parts remain to be considered—roughly Europe, the Middle East and Siberia. The focus is on Europe because it was the one place on earth that did not seem to need Professor

Greenberg poking around and upsetting things. One large phylum, Indo-European, dominated those parts, albeit somewhat challenged by Altaic. That large phylum (we will call it I-E henceforward) has been the database par excellence for modern linguistics throughout its development. As Ruhlen is fond of saying, much of linguistic thinking is 'Eurocentric'; so too has the classification and reconstruction of proto-I-E dominated the theory and practice of historical linguistics.

While there have been numerous attempts to find linguistic kin for I-E in various parts of the world, all such efforts have been fought off or simply lapsed through being ignored. Or had been until Illich-Svitych came on the scene in the 1960s in Moscow. We have mentioned the Muscovite efforts above. For now it is enough to say that I-E was put in a genetic group that included Uralic, Altaic, Japanese, Korean to the east and Kartvelian (South Caucasic), Dravidian or Elamo-Dravidian, and Afrasian to the south. Early on it became apparent that the new super-phylum, Nostratic, did not have an accurate internal taxonomy, that Afrasian stood partly aside as a coordinate sub-phylum, and that the relationships to I-E were not well-established. Was I-E a western Nostratic entity, like Kartvelian and Dravidian, or was it closer to Uralic and Altaic to the east?

Indeed it was time for Professor Greenberg to poke around in this matter. One of his first determinations was that we needed to find 'valid taxa', i.e,, those genetic groups closest to each other, even if related to others outside of that group. Thus Semitic, for example, was most probably related to I-E but neither of them were in the same valid taxon; so Semito-I-E was not itself a valid taxon. But Afrasian was a valid taxon and Semitic belonged in it. I-E did not. But instead of looking only at I-E as everyone had been doing Joe followed his own custom of looking at an area to find the valid taxa in it. Instead of being Eurocentric, he looked at the whole range of north Eurasian languages. Unlike the Nostraticists, Russian and American, he chose not to restrict the inquiry to language groups that had been well reconstructed.[13] He must have asked himself—how did I ever do Africa without reconstructions?

The result of Joe's search for the valid taxa[14] was to find a taxon to which I-E and nine other groups belonged—before they related to any outside groups. Thus I-E was more closely related to any of them—for example Ainu or Aleut—than it was related to Kartvelian or Semitic. From a taxonomic standpoint it was a neat solution, because this

---

[13] This is an important component in the debate between the Taxonomy First and the Reconstruction First schools of thought. As we will see below, the Russian position was an Indo-Europeanist's.

[14] It might be clearer to call them 'natural' taxa instead. Thus Dutch, Swedish, Portuguese, Sicilian, and Greek form a natural taxon—I-E—but the first pair and the second pair form two more natural taxa within the larger one, while Greek is by itself. Essentially, the whole discussion about valid or natural taxa is a sub-grouping problem. It would not occur if the languages under discussion had not been related to each other in overall terms.

Eurasiatic group formed a line across northern Eurasia and had some interesting properties.[15]

Nevertheless, Eurasiatic was a shock to European sensitivities. First, it was not closest to the old civilized peoples of the Near East. Second, it was closest to people who were physically Mongoloid, *i.e.*, Orientals such as Japanese and Mongols. This immediately made no sense in prehistory because there was no correlation between people of that appearance and I-E languages. But there was a high correlation between people of European or Caucasoid appearance and I-E languages. Clearly, either somebody had changed their language in ancient times or Greenberg was mistaken. But even if we went back to the old Nostratic, some explanation was needed for the disparity between western Nostratic and eastern in biological terms. Finally, recent DNA studies make it very clear that the phenotypes of Europe are genetically determined, not due to modern climatic factors, and that neighboring peoples to the south and southeast were their closest relatives, rather than the peoples east of the Urals.

Cases where people of different languages exchange genes are common in the world. Cases where a population has changed its language but not its biology or not most of its biology are less common in the world. Modern Egyptians speak Arabic but their biology is largely derived from their Egyptian-speaking ancestors. The Ainu have finally lost their language and incorporated many Japanese genes. The Hungarians and Turks kept their languages but were absorbed by local European populations. Those ethnic groups of New York City gradually become mostly English speakers. But the more common case is where populations exchange genes and words, each becoming different from what it was but usually recognizable in physical and linguistic terms. At the moment no one has proposed a good solution to the I-E problem outlined here.

Another surprise of Greenberg's Eurasiatic has been that in recent versions of it he has incorporated Etruscan, the great mystery of old Italy. Although its precise taxonomic position was not completely clear because of translation problems, Joe thought it either a separate branch of Eurasiatic as a whole or a sister language to I-E. His final taxonomy is presented below:[16] Eurasiatic was most likely related to other proposed members of

---

[15] From a phonetic standpoint I-E was like the rest of Eurasiatic in lacking glottalized consonants, pharyngeals or the retroflex sounds (found in Dravidian), although the Indic branch of I-E had acquired the retroflexes. The contrast with heavily glottalized Kartvelian and Afrasian is striking. It is perhaps not an accident that some Muscovites pioneered the reconstruction of proto-I-E as a glottalizing language. Joe never accepted that.

[16] This taxonomy is taken unchanged from pages 279-281 of Joseph H. Greenberg, 1999. *Indo-European and Its Closest Relatives.* Stanford University Press, Stanford, CA. [This lengthy taxonomy is left out of this 2013 edition, since it is readily accessible in the source cited, or on the Internet, e.g., the Wikipedia article "Eurasiatic languages." Ed.]

Nostratic, viz. Kartvelian, Dravidian, and Afrasian, but with the internal taxonomy of Nostratic unspecified. Also Elamitic (suggested by the Muscovites) and Sumerian (suggested by Bomhard) were likely members. Eurasiatic proper had this membership:

AN APPRAISAL

When a great controversial figure comes up for historical review, two things are usually apparent. First, she/he is not likely to be as bad as the critics maintain; second, she/he may not be as good as the apostles and friends say either. That meaning of the term appraisal will have to wait for the historians or wait for the field of linguistics to settle down a little. And becoming more tolerant would not be a bad idea either.

However, being too old to wait for the mills of the historians to finish their grinding, I state my opinion; it is already obvious. Joe Greenberg is like two other great scientists whose appraisals have been mostly finished—Charles Darwin and Alfred Wegener. Both had careers similar to Greenberg's. Remarkably creative hypotheses that were crucial to the growth of their respective scientific fields but associated with vociferous, sometimes savage, criticism, nearly to the point of anathema and banishment from the scientific community, or the civilized Christian community (Darwin). We can leave Darwin's case rest in the archives because it is so well known. For Alfred Wegener things are rather different because his theories were not a threat to the dominant religion of his time and because his story happened in the 20th century. He transformed geology, or the earth sciences if you prefer, by proposing the **theory of continental drift**, which nowadays can be heard on the evening news explaining things as different as the earthquake potential of California to the steady upward rise of Mount Everest year by year. Wegener was right; his hypothesis was correct. And almost all of his contemporaries who scorned or ignored him were wrong.

In my youth, when people who lived during the First World War were still numerous, there was a favorite saying: "Forty million Frenchmen can't be wrong!" It reminds me of one of the favorite sayings of contemporary linguists (e.g., Ives Goddard, *et al.*) that since 'mainstream' linguists disagree with Joe, he (Joe) must be wrong. The Wegener and Darwin cases suggest that the 'mainstream' can be wrong and surely is wrong quite often. Because, you see, there is no real scientific logic to either the 'mainstream' or the 'forty million Frenchmen' argument. The entire populations of Texas and Florida may believe that the 'lost continent of Atlantis' has been found in the Caribbean Sea. But those 34 million opinions are like the smoke in the air over Houston: a good rain will wash it away. What will determine the debates over competing hypotheses will be the data and analyses that accompany them and test them. For that is how science functions in the long run; temporary passions and biases slowly but surely lose out. Racist theories about human differences were in vogue, dominant, 'mainstream', a century ago. Yet they did not survive a hundred years of anthropological research that destroyed their credibility. True, millions of white people still believe in their innate superiority, but those opinions are considered simple bigotry. The mainstream moved away from them. Yes, sometimes the mainstream is right!

That being the case, we must move to the argument proper, instead of popularity contests. What are the empirical and theoretical issues between Greenberg and his Americanist critics? Before tackling them, however, we have to face a fact. We cannot resolve this dispute the way physics could; we cannot experiment and we cannot work it out mathematically.[17] But remember that Darwin's and Wegener's hypotheses were basically prehistorical problems, not quite like those of physics. Each had a key problem that could be addressed by controlled or focused observations. Darwin had two key general questions: (a) is there evidence of evolution, i.e., can any species change into another? and (b) what evidence is there from the past that some species have changed? Wegener's dual question could be put as: is there evidence that continents move or have moved in the past? Besides, the fact was that much of the evidence for either Darwin's or Wegener's theories was physical, biological, and sedimentary stuff. Furthermore each could find material evidence preserved from the past in the form of fossils and/or rock formations. Except for the late-occurring writing in a few areas, linguists were stuck with contemporary evidence of socio-psychological or cultural events.

Yet the three fields were not so different as one might think. They created evidence of the past by hypothesis. While paleontologists and archeologists regard their prehistoric data as solidly factual, we know that is not entirely true. One gazes at a slab of rock and concludes that trilobites lived here several millions of years ago. That is not a fact; that is a hypothesis. A colleague of mine once looked at a bone at a site in Kenya and called it a cow, thus exciting everyone. His 'fact' was later found to be a native antelope, much to his chagrin. A linguist looks at French *chien* and Italian *cane* (among other words) and concludes that they come from the same ancestral word, something like **kian* or **kan*, for 'dog'. He too has created a prehistoric fact by hypothesis. His 'facts' are called 'reconstructions', but they are also based on preliminary hypotheses—that *chien* and *cane* have a common ancestor—which usually are called etymologies or related forms.

Now we have come to an important difference between Greenberg and his critics (both American and Russian). Joe and his critics both start with basic facts, i.e., the words, phrases, and sentences recorded for each language and written down in interpretable symbols. In other words what most of us call the 'data'. When a linguist establishes her data base in several languages, she then begins to compare the sets of data with each other. Straight away, however, differences in approach occur. Some Russian linguists will set aside or disregard data from a language if its ancestor has not been 'reconstructed', believing that the quality of the facts is more important than their existence. Many American linguists, while not disregarding unreconstructed data, will still regard that kind of data with suspicion. Both share a belief that basic facts are not as reliable as reconstructions. Many contemporary theoretical linguists regard the basic facts as unreliable because they are 'realized' versions of the true 'underlying' facts. The attitude is amazingly similar to Indic religion in its belief in *maya* or sensory data as 'illusion'. To them the truth must be found behind the surface data.

---

[17] Thanks to Murray Gell-Mann for pointing out this difference between physics and historical linguistics. He made this point during a conference at the Santa Fe Institute in December, 1997. Contemplating the squabbling among linguists, he said: "We can't have this kind of problem for very long in physics because someone will make an experiment and settle the matter."

As mentioned above, Greenberg was a traditional or old fashioned historical linguist. He took the basic data from every language, whatever the condition of its recording, and compared it with the others in the region he was working on. When possible he searched for old sources (or those in different scholarly languages) in order to get more basic facts on any particular language. His famous method of 'mass comparison', lately called 'multilateral comparison', was grounded in a dislike of 'binarism', comparisons involving only two languages and not the whole available set. To Joe it was more important to confront all the phenomena than settle for a refined pair.

So we have the basis of the first set of criticisms. Greenberg used poor data and overlooked some of the finest reconstructions in existence, said Americanists. Greenberg did not use phonetic precision, said some English critics, many of whom were pre-phonemic in their thinking and often ignorant of standard I-E procedures. Joe basically shrugged, partly because many etymologies had been established long before the modern high-quality analyses had been made. Much of this criticism was grounded in the I-E dominance of most linguistic departments. Stemming ultimately from German high standards in culture, eventuating in the Neo-Grammarian demand for exact correspondences without exception and culminating in American theoretical schools adopting that demand, linguistics became obsessed with the demand for 'rigor', precision and nearly mathematical exactitude.

The next criticism found his critics putting carts before horses. It was that Joe proposed etymologies (cognations) that were not grounded in precise reconstructions. That is, one has to reconstruct the ancestral forms (words, grammemes) before proposing the relationships. But in fact one cannot have any reconstructions before one has established etymologies. First, one must propose that *chien* and *cane* have a common ancestor; then one may propose sound correspondences (like French *ch* often corresponding to Italian *c*), and then one can reconstruct the ancestral **kan-*. Eminent theorists of I-E reconstruction technique, such as Hoenigswald, missed this point and for a good reason. They were used to having the etymologies in hand because of I-E reconstructions. Had most of them worked on Amerind or African languages in the field they would have realized the obvious:

no established etymologies, hence no reconstructions, were available. One had to make them up out of raw data!

Furthermore, one could not make accurate reconstructions until one established priorities of relationship among languages being examined. If we agree that French *chien* and Italian *cane* are cognate, but also join German *Hund* to that etymology (cognate set), then the reconstruction will change. But if we realize that French and Italian are part of a special set in which German is not involved, then we can still get **kan-* for French-Italian and something else for German-French-Italian. That is, first we find the Romance level and then the I-E level.

All this is based on the historical flow chart that is sub-classification. Those in the same sub-group have shared historical experience peculiar to them. French and Italian were descended from Latin, the dominant part of Italic which came from PIE (proto-I-E). Latin had altered the PIE word for 'dog' into *canis*; that was the source of **kan-*. German on the other hand, along with English and others, had a different history. Their ancestral word was **hund*, itself from original PIE **kwon*. At root each of these groups came from the same language but their individual descent lines made a difference in reconstruction. At root one can say that those who do not have an accurate internal taxonomy for a family are not likely to get as accurate reconstructions as they could with good taxonomy.

Such is the basis for the 'Taxonomy First' argument, as opposed to the 'Reconstruction First' group. The argument is in many ways a flat-footed difference between Indo-Europeanists and more practical minded scholars from the realm of unwritten languages in Africa, Oceania, and the New World. Again one can ask what portion of the languages of the world have written ancestors—as many do in Europe—against which to check reconstructions? The presence of old written languages has been a major factor in the evolution of I-E ways of doing things, as it has also in Semitic. But I-E and Semitic number maybe 200 languages out of a world total of 5000 to 6000 depending on whose count one takes. That is to say, 3% or 4% of the world's languages should establish the methods, the strategies and tactics, of historical linguistics? Why?

Another bone of contention between Greenberg and his critics has been the time frame of possible classification and reconstruction. It would appear that some Americanists have made up out of whole cloth a cut-off time of 6000 years, plus or minus a millennium or two for different 'theorists'. The reason given for this cut-off is that after such a length of time the evidence of relationship would have disappeared or become insufficient for accurate work. As far as I can tell, the first Americanist to propose this theory was Terrence Kaufman.[18] But ultimately the trail of this theory goes to Winfred Lehmann at Texas and then to its apparent source in an article that M.L. Bender wrote in 1976. That article, based on glottochronology or lexicostatistical tables, calculated the amount of vocabulary two languages would share after so many millennia. Bender's undergraduate degree was in mathematics (Dartmouth) and he was appalled by the low retention after 10,000 years (1%) or even 6000 years (7%). How could anything substantial be left to work with when over 90% of the vocabulary had been lost? He asked. So he and others such as Lehmann generalized the lexicostatistical conclusion to mean that most of the evidence of relationship was gone after 10,000 years or sooner.

---

[18] Unfortunately, I cannot find the original source. My knowledge is actually based on a personal communication from Kaufman. Pittsburgh, 1991.

It was a tremendous mistake in reasoning that was quickly pointed out to Bender[19] before he published the article. Nevertheless the article was published. We should have shouted it down but we were too busy. What were the mistaken assumptions?

1) The automatic presumption of a binaristic scene. Had he not done so he would have realized quickly, being a good mathematician, that each new language added to the comparison increased the number of common retentions left over. So if three, rather than two, were compared, then 3% would be left after 10,000 years instead of 1%. If four languages, then 6%; if five, then 9%; and so forth until twenty languages yield 62% after 10,000 years.[20]

2) The assumption that, since the Swadesh list of 100 or 200 words was the most conservative vocabulary in any language, the loss in the rest of vocabulary would be even greater. That is probably true. But he forgot that the general lexicon is far more numerous than the Swadesh lexicon. Just suppose that general lexicon is 'lost' twice as fast as basic vocabulary. So after 10,000 years two languages would have only one word retained in common, or 1% of 100 words. Therefore there would only be 0.5% of general vocabulary left. Yes, but general vocabulary probably consists of 5,000 or 10,000 words. So 0.5% of that would be between 25 words and 50 words after 10,000 years. That is still binary, between two languages only. More languages would yield more; for example, three languages for 10,000 years would yield 75 words. But twenty languages at 5,000 years

---

[19] Explicitly, Paul Black (Yale PhD 1975) and I reacted strongly and with dismay to Bender's article. Black wrote a nine page critique and I sent a shorter one. Bender's conceptual errors were blatantly obvious.

[20] These percentages are taken from Table A.2. "Recoverable Vocabulary Based on a Homogeneous Replacement Rate." In Joseph H. Greenberg, 1987. *Language in the Americas*. Appendix A, 341. Stanford University Press, Stanford, CA. Allowing for the Joos function or the 'dregs effect' yields even higher percentages. The reader is warned, however, that a few errors exist in the tables at the higher ranges (years) due to simple clerical mistakes.

would yield 1,550 words.[21]

3) The misconception of what 'loss' or 'retention' meant in the Swadesh list. He forgot that words are 'on' the list when they are the dominant form in a language. But they may still be in the language and not even far away. There are many examples of this, as between English and German, for example. Nowadays English *dog* and German *Hund* are on the list, so the native English form was 'lost'. But it is still there as *hound* for hunting or sporting dogs. The same for *bird* in English whose older *fowl* is still in the language and cognate with German *Vogel*. Sometimes, of course, a word remains in the language, although it is lost until reconstruction or at least good etymologies have been made. Thus German *klein* for 'small' is cognate with English *clean*, while English *small* is cognate with German *schmal* 'narrow'. This leads to the well-known rule that as etymologies, sound correspondences, and reconstructions increase in number they make possible the discovery of more 'lost' words.

4) Most meaningful of all was the misunderstanding of what genetic classification consists of, or at a minimum what Greenberg did when he classified languages together. As is well known, there is a streak of extreme preference for grammatical evidence among Semiticists and Indo-Europeanists. Of course, not all share these extreme predilections in those fields, but their representation is numerous. Perhaps my friend, Robert Hetzron, was a prime example among Semiticists. Anecdotally, we hear that the inclusion of Celtic within I-E was held up for a long time because Celtic lacked 'crucial' morphological evidence.

Against that background we must realize that some scholars under the influence of Swadesh developed a strong preference for the lexicon, i.e., for words rather than grammemes. How else to account for Bender, Kaufman and others who used only lexical information to establish the alleged time frame or time limit of 6000 to 10,000 years? Yet all one has to do is look carefully, or to scan rapidly, Joe's African or other classifications to see that he *always* began with grammatical evidence in setting up his genetic groups.[22]

The principle can be stated quite clearly. Two or more languages can be classified together when the investigator finds enough etymologies involving basic vocabulary, general vocabulary, and grammatical morphemes (grammemes) to convince her that these languages had a common ancestor. Arguments from syntax, phonology, racial similarity and typology have turned out to be unreliable and misleading, so Joe didn't use them. Even such a striking thing as the presence of click phonemes does not necessarily lead to valid taxa, as the vivid case of South African Bantu languages can testify.

---

[21] See *Mother Tongue* (Newsletter) 24 for a longer discussion of this point, including the retention possible with 40 or 80 languages.

[22] Only Chinese and languages like it prevented this procedure, as Greenberg repeatedly acknowledged, because there was a lot of syntax but not much morphology. Only recently with George van Driem's work on Sino-Tibetan pronouns have we broken out of that bind.

Even if the Swadesh retentions get very low, it does not follow that no evidence of relationship is left. That remains an empirical question, not one to be decided by lexicostatistical theorizing.

From this Greenbergian view it has been curious that Paul Benedict was able to hold up the achievement of phylum *Austric* because it "only had morphological evidence." So any Semiticist would have said that was fine and dandy! What is obvious about Austric with so little vocabulary evidence (allegedly) is that it must be very old, comparable to the African phyla with their low percentages of lexical retention.[23] But L.V. Hayes *does* find lexical evidence.

While many minor objections to Greenberg's work no doubt exist, the major ones seem to have been addressed. One remaining question is: why such vehemence, such fury, in attacking a mild-mannered scholar trying to help science understand our common prehistory? Is Cavalli-Sforza right in stipulating 'jealousy' as a motive for the attackers? Was this the same syndrome as that displayed by the Algonkianists in halting Sapir's work? Or is this just normal science in a field that cannot do experiments? It seems clear that Greenberg represented a throwback to an older paradigm of historical linguistics, an older Americanist and 19th century Indo-European tradition that threatened the new high-tech, rigorous, theoretical paradigm trying to establish itself as linguistics.

Next year, and probably sooner, a conference will be held to address many of these Greenbergian questions. Let us hope that the historians of science, as well as philosophers of science, become interested in the topic. It is truly interesting.

* * *

References:

Bibliographical items have been kept to a minimum (see footnotes). I thought it more valuable to outline the issues, knowing that the specific authors and writings were fairly well known, than to produce five more pages of dense citations. HCF.

---

[23] When Sheila Embleton and the rest of us get linguistic dating back on its feet, we will probably find that Austric is closer to 20,000 years old; that just figures from the great age of Homo sapiens in Southeast Asia and the very low lexical factor. L.V. Hayes' etymologies then will be very valuable in our work.

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

Why Is Africa So Linguistically Undiverse? Exploring Substrates and Isolates

Roger Blench
Kay Williamson Educational Foundation, Cambridge, U.K.[1]

For the special issue of *Mother Tongue* celebrating the work of Joseph Greenberg

1. Introduction

One of the notable differences between Africa and most other linguistic areas is its relative uniformity. With few exceptions, all of Africa's languages have been gathered into four major phyla, and most recent progress in classification has been in resolving details. The number of undisputed language isolates is very small. By contrast, Australia, Papua and the New World are usually considered diverse at the phylic level and all have numerous isolates or very small phyla. Eurasia is hard to classify; Europe is undiverse and is characterised by a small number of geographically extensive languages, but NE Asia is a reservoir of small phyla. SE Asia, on the other hand, is very similar to Africa, in having few phyla, each with many languages and no isolates, if Andamanese is excluded.

Looking at the worldwide pattern of isolates, it is evident that they are very unevenly distributed. There is a gradient from west to east, with few in Europe and the greatest number in the New World. Probable Eurasian isolates that are long extinct, such as Sumerian and Etruscan, point to a period of greater diversity, but crucially, they have been assimilated. The high density of isolates in the Americas tells us something very significant about the proposed chronology of the peopling of the New World, namely that such richness cannot have arisen within the chronological constraints accepted by many North American archaeologists (Blench 2012). For so many languages to have been diversifying for so long as to eliminate all traces of links with neighbouring languages requires time-depths similar to those accepted for Papua and Australia.

Table 1 shows the different regions of the world and the numbers of isolates and small phyla, by my own count, but based on sources such as *Ethnologue* (Lewis et al. 2013), Campbell (1997a) and Pawley (2012). Totals do not include extinct languages, otherwise the numbers for the New World would be significantly higher. Total numbers of living languages come from *Ethnologue*:

---

[1] Kay Williamson Educational Foundation, 8, Guest Road, Cambridge CB1 2AL, United Kingdom | Voice / Ans (00-44)-(0)1223-560687 | Mobile worldwide (00-44)-(0) 7847-495590 | E-mail rogerblench@yahoo.co.uk | http://www.rogerblench.info/RBOP.htm

Table 1. Isolates, small phyla by continent

Continent	No. Isolates	No. Small Phyla	Total living languages
Africa	6	0	2146
New World	71	43	1060
Eurasia	6	1	2588
Pacific	12	4	1066
Australia	7	13	245
Total			7105

This view of diversity conflicts radically with the 'long-ranger' view of the world language phyla, whereby the New World can be rolled up into just three families and much of Melanesia into a single family, Indo-Pacific. The major issue is whether this reflects intellectual tradition or linguistic reality which is considered below. However, it should be noted here that Greenberg (1971, 1987) simply does not discuss the classification of many languages in the New World and Melanesia now claimed to be isolates by regional specialists.

Given the time-depth of human settlement in Africa, this situation of uniformity is somewhat surprising. If the *ex Africa* hypothesis for the origin of modern humans is accepted, then *Homo sapiens sapiens* originated some 150-200 Kya and spread to Eurasia from Northeast Africa, largely displacing, but perhaps also interbreeding with, the hominids already *in situ*. It is now widely accepted that modern humans evolved in Africa (Ke *et al.* 2001). The first evidence for archaic modern humans is in the Omo Valley some 195 kya ago (Macdougal et al. 2005). This are likely to have been three waves of hominin dispersals out of Africa between 1.9 and 0.7 Ma (Bar-Yosef and Belfer-Cohen 2001). Genetic and archaeological studies suggest that anatomically modern humans (AMH) dispersed out of Africa sometime between 130 and 50 ka (Gunz et al. 2009). Behar et al. (2008) have demonstrated that there was a major division in human populations some 70 kya leading to the genetic isolation of the Khoisan in Southern Africa. This must be connected with the finding that the ancestor of modern males, associated with mitochondrial haplotypes M and N, appears at this time. The claim is that M168 mutation was carried out of Africa and is characteristic of all non-African males. They associate this with the megadrought in Eastern Africa between 135 and 70 kya (Cohen et al. 2007).

The routes by which modern humans left Africa remain disputed. Cave sites in Israel at Qafzeh make it certain that one exit was via Sinai into the Near East. In the early 2000s, it was commonly accepted that modern humans left Africa via the Bab el Mandeb, the modern Horn of Africa, crossed to Arabia and followed the coastlines all the way down to Australia. This scenario found support from DNA analyses of Andaman Islanders and Orang Asli (Macaulay *et al.* 2005). Another likely dispersal route is across the Sahara, either via the Nile (Vermeersch 2001) or the 'green Sahara' during more humid periods in the past (Drake et al. 2011). There is evidence for AMH occupation of North Africa including the North African littoral, the Sahara and the Nile in the form of the Nubian and Aterian lithic industries (Van Peer 1998). These archaic humans, or similar peoples, could have dispersed out of Africa at around 120 ka (Dennell and Petraglia 2012) in the light of archaeological remains in the Levant (Grün et al. 2005) and the existence of Nubian technology in Arabia (Rose et al. 2011). However, a coastal is yet to be confirmed by excavation, despite considerable advances in the archaeology of Arabia (Petraglia 2007; Petraglia & Rose 2009). Current views are tending towards a model whereby foragers crossed the centre of the Arabian Peninsula at a period when the environment was considerably more favourable (Petraglia et al. 2010).

Given the antiquity of human evolution in Africa, the low number of isolates and lack of linguistic diversity clearly requires an explanation. A number of hypotheses can be advanced to account for this. The simplest is that languages diversify at non-uniform rates and therefore there is no puzzle. This is hard to disprove at some level, since it can be demonstrated for some historical cases. For example, the evolution of script systems appears to have a major impact on slowing rates of language diversification, as does the growth of state systems. However, none of these seem to be relevant in the present case, where these areas have remained populated by small, ethnolinguistically distinct groups without writing. If we accept there is a link between apparent diversity and time-depth, essentially three alternatives remain:

a) Africa was once as linguistically diverse as other regions of the world and this diversity has been eliminated by the expansion of the ancestors of today's phyla since the Holocene
b) Intellectual traditions concerning classificatory processes are significantly different in other regions of the world and the apparent disparity is illusory, e.g. Papuan, Australian and Amerind do form coherent, so far unrecognised phyla
c) *or* the classification of African languages is radically in error

Joseph Greenberg, whose classification of African languages undoubtedly formed the basis for all subsequent classifications, certainly focused on the second explanation. His arguments for Indo-Pacific (Greenberg 1971) and Amerind (Greenberg 1987) claim that most of the languages of these regions *can* be classified into large phyla[2]. The third alternative, that we are simply wrong about classification, can be associated with Africanist linguists such as Gerrit Dimmendaal (2011), Tom Güldemann (2008, 2011) and the global linguist Harald Hammarström[3], who suggest that Africa has many more small families than are currently accepted, and that we have failed to recognise this. Importantly, this is a claim that Greenberg (1963) is seriously in error. However, it is problematic to discuss their argument, since by and large Dimmendaal and Güldemann are not interested in historical linguistics. They assert that Mande or Songhay have been misclassified but do not present any argument against the traditional view.

---

[2] Although these ideas had precursors, notably the observations of Trombetti (1905: 205-208) who noted the prevalence of 1st person N- and 2nd person M- pronouns in numerous American languages, Greenberg was the first to take the bolder step of claiming these were a key piece of evidence for a continent-spanning phylum. Greenberg (1996) cites some of his predecessors in this area. The notion that these pronoun distributions are statistically significant has been questions by various authors, notably Campbell (1997b) and Brown (n.d.) who compares Amerind pronominal patterns with worldwide distributions.

[3] Hammarström's views are summarised at http://glottolog.org/glottolog/family which certainly represents the extreme end of the spectrum. Not only is Nilo-Saharan discounted, but major branches within it are dismantled. Omotic is similarly dismembered and several branches of Niger-Congo treated as isolated families.

This paper[4] assumes that Greenberg's overall picture for Africa was broadly right, but that his arguments for other linguistic regions of the world were at best over-optimistic. This is particularly the case for the Americas, where no recent papers by regional specialists have supported Amerind, and many have rejected it. The only supporting arguments appear in the work of Merritt Ruhlen[5] (e.g. 1994, 1995). Indo-Pacific is more complex, since some of the languages Greenberg grouped together *are* now considered to be part of the Trans New Guinea phylum (Pawley 2005, 2012). Whitehouse et al. (2004) further include the Nepalese isolate Kusunda within Indo-Pacific[6]. Andamanese and Tasmanian have failed to garner further support in the intervening period, despite a major expansion of data on Andamanese (Abbi 2006, 2012). Crowley & Dixon (1981) considered the evidence for a Tasmanian link within Indo-Pacific but came up with strongly negative conclusions. The overall assessment of the research community in relation to Greenberg's proposals can be measured by the establishment of scholarly conference series, which exist for Niger-Congo, Nilo-Saharan, Afroasiatic and Khoisan (i.e. all the African groups) but not for either Amerind or Indo-Pacific.

This paper examines the alternative explanations and seeks a synthesis to account for the current situation. However, it also asks questions about the modelling of linguistic diversity more generally and by inference how we assign chronologies and time-depths to phyla with no significant written records. This is clearly not just an artefact of the way we do linguistics, but somehow reflects a real phenomenon. The final section of this paper will consider why it should be that Africa, generally considered to be the original home of modern humans, should have such low linguistic diversity.

2. Methodological issues

2.1 Traditions of classificatory research in Africa and beyond

The perceived diversity of a linguistic region is not entirely the result of a rigorous scientific process; it also reflects strongly the patterns established in the early period of scholarship. In African studies, the intellectual tradition has been characterised from an early period by continent-spanning hypotheses. The discovery that Bantu languages from

---

[4] One section of this paper was first presented in Canberra on 30th July 1999 in the Department of Archaeology and Anthropology. A more complete version was circulated at the workshop 'Language Isolates in Africa' Lyon, December 3 and 4, 2010. For more recent materials on African isolates I'd like to acknowledge the help of Philippe Beaujard, Harald Hammarström, Abbie Hantgan, Maarten Mous, and Martin Walsh in making available and commenting on various materials. Work on data analysis was supported by the Kay Williamson Educational Foundation.

[5] Ruhlen re-edited the Amerind etymological dictionary after Greenberg's death (Greenberg & Ruhlen 2007).

[6] Kusunda was thought to be extinct, but surprisingly some speakers were contacted in 2004 and a grammar and wordlist have now been published (Watters 2005). These do not provide support for the link with TNG. Blench (2008) notes that Kusunda has a distinctive vocabulary for agriculture, suggesting they are not archaic hunter-gatherers, but a reversion from a farming culture.

Cameroun to South Africa were related dates back to the seventeenth century[7] (Doke 1961) and the discovery of noun-classes in West African languages led some nineteenth century scholars to speculate on their relation to Bantu. Wilhelm Bleek (1862, 1869) included a West African division in the family he named Bantu. Meinhof, one of the most influential figures in early twentieth century African linguistics, was originally a Bantuist and it is more than possible that the 'large-pattern' world view that this engendered influenced much of his later work, now considered misconceived, that linked together numerous languages in his 'Hamitic' hypothesis. Diedrich Westermann (1911) posited a 'Sudanic' family corresponding to Meinhof's work on Bantu, and proposed a division between 'East' and 'West' Sudanic, linking together what we would now consider Nilo-Saharan and Niger-Congo. Westermann's second major publication (1927) set out a large number of Proto-West Sudanic (PWS) reconstructions, broadly corresponding to modern Niger-Congo and playing a role analogous to the work of Dempwolff (1934-1938) in Austronesian reconstruction.

Joseph Greenberg took a fresh look at the classification of African languages in a series of articles published between 1949 and 1954, later collected in book form in 1963. He combined 'West Sudanic' and Bantu into a phylum he named Niger-Congo, while he treated 'East Sudanic' as a different phylum, renamed Nilo-Saharan. He renamed the 'Hamito-Semitic' languages Afroasiatic and while re-iterating the Khoisan hypotheses of Dorothea Bleek (1956), who had assumed not only that all the click languages were related to one another, incorporated the languages with clicks in East Africa, a hypothesis later named Macro-Khoisan. The effect of this was to tidy up the linguistic picture of the whole continent –every language was theoretically 'placed'. Why Greenberg seems not to have considered the possibility of isolates is unclear, but his approach has been enormously influential on succeeding generations of Africanist scholars. Indeed, Greenberg's later publications, first on Indo-Pacific (Greenberg 1971), then gathering all the languages of the Americas into three phyla (Greenberg 1987) and bringing together Eurasian languages into 'Eurasiatic' (Greenberg 2000), a version of Nostratic, show that he was a committed 'lumper'.

These views remain very much at odds with more conventional scholarly opinion on the languages of Papua, Australia and the New World. In these regions, linguists have generally entered the field with no preliminary assumptions about relatedness or macro-groupings –and so progress has been much more 'bottom-up'. Small groups have been derived from data and gradually built into larger ones. Pama-Nyungan and the Trans-New Guinea Phylum (TNG) represent 'entry-level' attempts to try and discern larger patterns, but they are far from encompassing the whole repertoire of languages and are still treated with scepticism in many quarters[8]. Similarly, the Amerindianist tradition shares much in common with Papuan. The earliest classifications, such as that of John Wesley Powell in

---

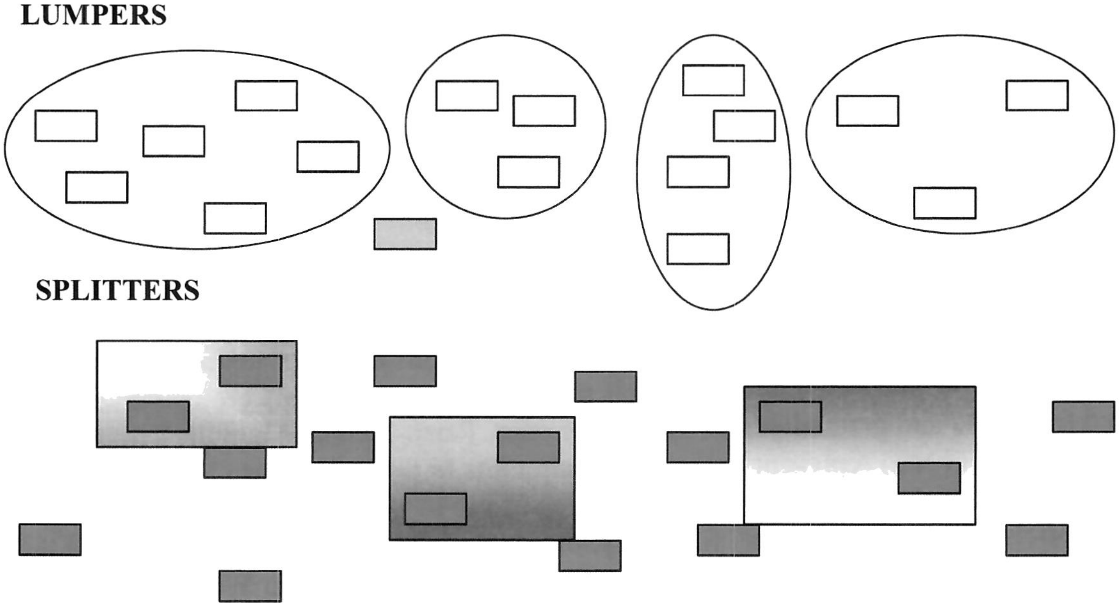
[7] Though an intriguing passage in Strabo, Geography Book II, Chapter 3 about the Greek navigator Eudoxus of Cyzicus (fl. c. 130 BC) suggests that the similarities of Bantu may have been noticed much earlier.

[8] See Pawley (2005, 2012) for a re-evaluation of the TNG.

the 1890s, divide Amerindian languages into numerous unrelated families. Although Amerindianists have gradually been reducing this number[9], they have essentially worked from the assumption that languages constitute distinct phyla until they are shown to be related (Campbell 1997; Campbell & Poser 2008). But, unlike Greenberg's proposals for Africa, Amerind and other constructs have gained very limited assent from the scholarly community. *Language in the Americas* was reviewed by a variety of scholars in *Current Anthropology* 28.5 (1987) with rather mixed results. More tellingly, the individual datasets were reviewed by numerous specialist scholars in the early 1990s in the *International Journal of American Linguistics*. Like the headline review (Rankin 1992), almost all were resoundingly negative. Greenberg (1996) mounted a spirited defence of Amerind claiming in effect that a small number of inaccuracies did not constitute a disproof of the hypothesis. However, a point made by many discussions is the inadmissible segmentation of words, leading to pseudo-cognates, i.e. claiming incorporated morphology in one language is cognate with a stem in another, and there is no obvious response to this. More recent discussions have not necessarily improved the picture (e.g. Rankin 2012).

The early tradition within a region is thus extremely powerful in determining the pattern of phyla, families and isolates. If Africa were in Melanesia, as it were, its linguistic geography might well be a few larger phyla and many isolates characterised by complex contact phenomena. Figure 1 is a schematic model of lumpers and splitters which tries to visualise the impact they have on our perception of the linguistic geography of a region.

Figure 1. Schematic model of lumpers and splitters



---

[9] Not always. Arawakan has 'lost' languages since earlier reconstructions, since it was realised Harakmbet is unrelated.

Is the conclusion that we might also be wrong about Africa? That Nilo-Saharan, Niger-Congo or Khoisan are no more than networks of isolates, or much smaller phyla, and the supposed cognates simply borrowings or chance. If we depended only on Greenberg's 'mass comparison' this might indeed be the case, since it is now all too apparent how significant borrowing can be between languages. But with the exception of Khoisan, our existing phyla are probably safe, because Greenberg depended on a body of prior work. Although he rarely cites it, it provided much of the evidence underlying his proposals. The comparative nominal morphology of Niger-Congo was first laid out in detail by Westermann (1935) and it is largely one of the accidents of history that this is not regularly cited as the key paper in establishing the phylum. Recent publications (beginning with Dixon 1997 but characterising some of the papers in Heine & Nurse 2008) and conferences, such as 'Beyond Niger-Congo' have used geographical and typological mapping of traits to suggest that Niger-Congo in particular is somehow not a valid phylum. This type of argument is problematic for two main reasons. First the authors rarely engage with the literature, failing to describe the errors that presumably characterise the proposals of those who want to argue for the reality of particular phyla. But more important it represents a methodological error, the assumption that demonstrating contact phenomena or mapping typological traits constitutes an argument against genealogical affiliation.

As an example of this type of construct, consider 'Macro Sudanic' (Güldemann 2008, 2011). This consists of a series of maps and tables showing that particular phenomena (labial-velars, logophoricity, vowel harmony) have quite similar distributions across a wide area of northern Sub-Saharan Africa. Güldemann concludes from this that the historical linguists are wrong and that 'the Macro-Sudan belt is genealogically highly heterogeneous'. In other words, this is a reprise of the arguments of David Dalby, who plotted a 'Fragmentation Belt' across Africa. But evidence that widespread phenomena are partly attributable to genealogical characteristics of language and partly to contact does not discredit historical linguistics. Indeed it is hard to imagine what type of scholar would not accept such a model. The point, and it is an important one, is that some linguistic phenomena are more prone to diffusion than others.

Even one of Güldemann's own examples, the labial flap, does not seem a very convincing demonstration of his point. The labial flap has a rather discontinuous distribution within Africa, found in some languages and not others. As Olson & Hajek (2003) show, there are 'islands' in Southern African Bantu, very remote from the main region of distribution. How are these to be interpreted? Was the labial flap part of proto-Bantu and has simply died out elsewhere? This seems somewhat unlikely. Did it arise independently? Given its global rarity, again improbable. It turns out that the labial flap is far more common than is suggested in the 2003 survey (see additional material in Anonby 2007). Matthew Harley (p.c.) reports it in several languages in central and northwest Nigeria, including Tərin [Pongu]. As an example of how it can be simply not heard because it is unexpected, take the example of Bafut, a Grassfields language. Despite its clear presence, previous accounts of Bafut phonology fail to recognise it (Mfonyam 1989). The likely explanation of its presence in Shona is that it *is* scattered across Bantu; not a reconstructible phoneme, but carried by marginal lexicon, for example in ideophones, and simply not recognised in many linguistic descriptions. This also exemplifies the issue of defects in the descriptive literature. Güldemann maps the distribution of ±ATR vowel harmony as it was known around 2000. However, it turns out that much of the literature on Nilo-Saharan was inaccurate, and that, for example, the Kadu languages and Fur also have vowel harmony. Linguistic geography is highly contingent; it depends on the phenomena you decide to map, the literature you consult, and the state of linguistic description, which itself may reflect politics and financial resources. It has little to do with the argument about whether cognate morphemes in Niger-Congo affixing systems constitute proof or otherwise of the reality of the phylum[10].

2.2 Misclassification of marginal languages

Any reasonably complex linguistic situation tends to present a mosaic of languages, some that show strong links with their neighbours and those that are less easy to classify. At least in Africa, such difficult to classify languages have a correlation with small, remote, marginal or isolated groups. The implicit assumption of most historical linguists has been to group the clearly-related languages, name them and then assume that the others must fit in somewhere. The evidence for this has all too often been a rather small number of common lexical items –an approach pioneered by Greenberg in 'mass comparison' and rehearsed in all too many documents since.

To give some concrete examples, the Fali languages of Northern Cameroun were classified by Greenberg as a group of Adamawa Eastern (now Adamawa). Lexical, grammatical and survey data is available for all these languages (Sweetman 1981a,b) and Fali has no single lexical or morphological feature that links it unambiguously to Adamawa, although it 'ought' to be Adamawa in terms of linguistic geography. However, the inertia of classification is such that no author has come out and simply asserted that it is 'not' Adamawa. Another language misclassified by Greenberg as Adamawa, Chamba Daka, has been excised and is generally considered Bantoid (Boyd 1994). This type of negative argument is much harder to make than one for a positive affiliation and probably less intellectually exciting, especially if you have no alternative proposal. Similar are the Ega language of Cote d'Ivoire, regularly cited as 'Kwa' despite a maximum 9% lexicostatistic resemblance to other purportedly related languages and Seme, in Burkina, Faso, which shows a similarly low level of resemblance to Kru.

A parallel can be drawn here with the classifications of Indo-European. We tend to think of Indo-European as a widespread phylum with a small number of well-known languages, Latin, Greek, English, Sanskrit, etc. However, there are well over 400 Indo-European languages, the majority of which are spoken in India. Some of these have intriguing and unusual features such as elaborate tone systems and complex phonology, as well as exhibiting considerable lexical diversity. However, they are all classified as 'Indo-

---

[10] Larry Hyman (2011) has also presented a detailed critique of Güldemann's methods and results, although using very different examples from those given here.

Aryan' on the grounds that 'tribal' languages are merely 'dialects' of the larger languages and that those in turn can be related back to Sanskrit (e.g. Masica 1993). There is no material at all on the sources of the non-standard vocabulary. These languages could just as easily be the non-Indo-European speech of resident hunter-gatherers relexified through contact with the vernaculars of incoming agriculturalists. However, the pattern of scholarship and the non-availability of key data on specialised lexicon makes this speculation difficult to pursue.

3. Language isolates in Africa and elsewhere

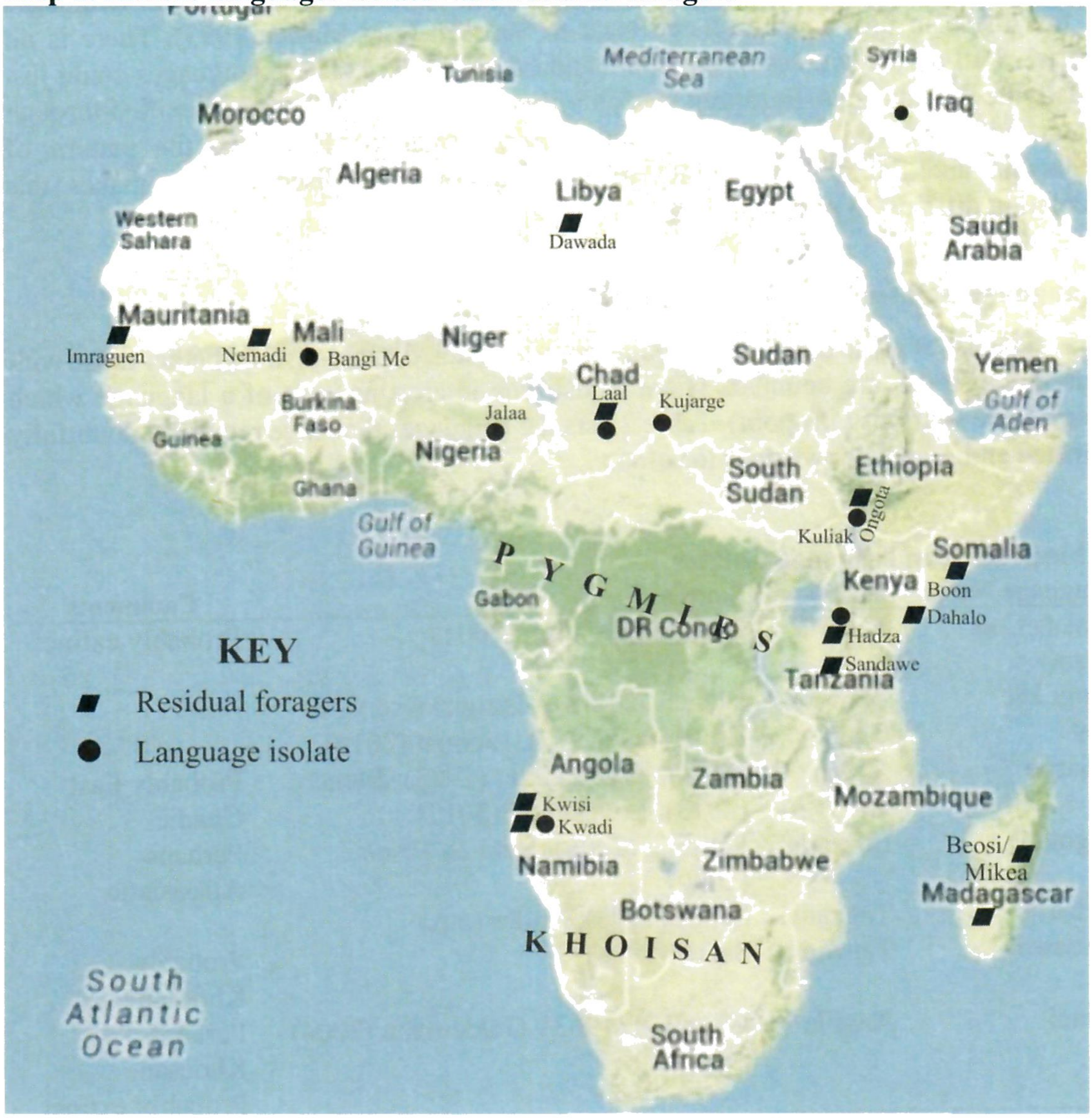
The list of African isolates remains controversial, depending on whether those with apparent substrates are counted. There is also one interesting case of a language which appears to be spurious (Oropom). Table 2 lists the languages that have remained doubtfully classified and Map 1 shows their locations:

Table 2. African language isolates

Language Name	Location	Source	Comments
Jalaa (=Cuŋ Tuum)	Nigeria	Kleinwillinghöfer (2001)	Probably extinct
Bangi Me	Mali	Blench (2007a), Hantgan (p.c.)	
Laal	Chad	Boyeldieu (1977), Lionnet (2010)	
Kujarge	Sudan	Doornbos & Bender (1983); Blench (2013); Lovestrand (2012)	Probably East Chadic
Ongota	Ethiopia	Fleming (2006), Savà & Tosco (2000)	Perhaps Afroasiatic
Hadza	Tanzania	Sands (1998), Miller (p.c.)	
Sandawe	Tanzania	Sands (1998)	Probably Khoesan
Kwadi	Angola	Westphal (1963), Güldemann (2004)	Perhaps Khoesan. Probably extinct

Of these, it seems likely that Kujarge and Sandawe are respectively Afroasiatic and Khoisan, giving a total of six.

Map 1. Africa: languages isolates and residual foragers


Portugal
Tunisia
Mediterranean Sea
Syria
Iraq
Morocco
Algeria
Libya
Egypt
Western Sahara
Dawada
Saudi Arabia
Mauritania
Mali
Niger
Imraguen
Nemadi
Bangi Me
Sudan
Yemen
Chad
Laal
Kujarge
Burkina Faso
Jalaa
Gulf of Aden
Guinea
Nigeria
South Sudan
Ethiopia
Ghana
Ongota
Gulf of Guinea
Kuliak
Somalia
PYGMIES
Kenya
Boon
Gabon
Dahalo
DR Congo
Hadza
KEY
Sandawe
Tanzania
▰ Residual foragers
● Language isolate
Angola
Zambia
Mozambique
Kwisi
Kwadi
Beosi/Mikea
Namibia
Zimbabwe
Madagascar
Botswana
KHOISAN
South Atlantic Ocean
South Africa

There are number of languages which have been reported initially as isolates, but which seem to be affiliated to known phyla, as shown in Table 3:

Table 3. African language reported isolates

Name	Location	Source	Comments
Bēosi	Madagascar	Blench (2007b), Blench and Walsh (n.d.)	Austronesian with unknown ? Southern Cushitic substrate
Dompo	Ghana	Painter (1967), Blench (n.d. b)	Guang language with unknown substrate
Gumuz	Ethiopia	Bender (1979), Ahland (2010)	Nilo-Saharan isolate branch
Mbre	Cote d'Ivoire	Creissels (n.d.), Blench (n.d. c)	Niger-Congo isolate branch
Mpra	Ghana	Cardinall (1931), Blench (n.d. a)	Extinct. Kwa language
Oropom	Uganda	Wilson (1970)	Probably spurious
Shabo	Ethiopia	Bender (1983), Fleming (1991)	Nilo-Saharan isolate branch

4. Substrates and isolates: their definition and detection

4.1 Where does substrate vocabulary show up?

If it is the case that Africa was once substantially more diverse, do traces of this diversity persist? In particular, if foragers became farmers and lost their original language in the process, under what conditions should some elements of it remain in their speech? Global evidence for the persistence of substrate vocabulary is very mixed. Evidence from place names (including those recorded in Ptolemy's Geography, 2nd Century AD) suggests that British Isles was once populated by populations speaking a language of unknown affiliation, Pictish (Jackson 1955). Some inscriptions in the Ogham script are untranslatable (Lockwood 1975) while others are clearly Celtic. But very few items of substrate vocabulary remain in English, and these can be identified principally by our comparative knowledge of surviving Celtic speech and literary records. In regions of the world where we have no such records, fragmentary vocabulary such as this would be extremely difficult to identify. By contrast, classical Greek is considered to have just 22% of its lexicon traceable to Indo-European roots, and the remainder presumed to derive from pre-Greek languages, 'Pelasgian', which were almost certainly non-Indo-European (Heubeck 1961; Beekes 2010).

It is therefore likely that these differences are related to the sociolinguistics of language assimilation. Powerful states which impose their languages on new subject populations are more likely to insist on language replacement. Slow language shift is more likely to produce lects which retain the lexicon and sometimes the phonological and morphological structures of previous languages. A good example of this in Africa is Yei, a Bantu language spoken in the Caprivi Strip. It retains or has borrowed a substantial number of 'click' words, especially to do with plants and animals, characteristic of the Khoesan languages in this region which it must have replaced (Sommer & Voßen 1992).

Interestingly, these words retain their original click status rather than being phonologised; hence Yei has a very large inventory of rare sounds.

The detection of substrates requires, above all, a rich descriptive literature. Substrates are likely to survive in specialised lexical fields, for example animal and plant names, hunting and fishing technology, religious terms. It almost always will also be present in morphology and syntax, but these are always much harder to interpret. For example, in the Kadu languages of Kordofan, a relatively coherent, closely related set of languages, two of the nine lects are SOV instead of SVO (Hall & Hall 2004; Blench 2006a). This is surprising, given that most African language families are highly conservative about word order. Is this change in Kadu due to the influence of a substrate or bilingualism in neighbouring languages? Both word orders occur in the region. Or is just an internal change, part of language evolution? The arguments are lengthy and often unresolved, which is generally not the case with distinctive technical vocabulary.

4.2 Chance? Can we exclude fortuitous resemblances

The identification of isolates depends on the tools used to classify languages. If a language shows only a small number of problematic cognates with its proposed relative, then its genetic affiliation will inevitably be questioned. Nilo-Saharan and Khoisan in particular include languages whose inclusion in the phylum remains debated. Several of the languages of the Ethio-Sudan borderland, such as Shabo and Gumuz within Nilo-Saharan (e.g. Bender 1979) and the 'Mao' languages, particularly Ganza, within Omotic, have very low lexical cognate count with their relatives (Bender 2003). If we claim that a substrate in a language can be identified in the lexicon of a quite different language, what counts as proof of lexical resemblance? Three explanations are possible:

a) the putative branches have been diverging away from the rest of the phylum for sufficiently long for vocabulary erosion to be responsible for low lexical counts
b) apparent similarities with the other branches of the phylum are due to borrowing
c) chance

Linguistic analysis, the demonstration of regular sound-correspondences or the detection of loanword phonology should be sufficient to show whether a) or b) are probable. But what about chance? There is a literature suggesting that lexical lists of any two languages in the world might show up to 5% resemblances of CVC stems (Bender 1969). Calculations by Ringe (1992, 1999) have applied a great deal of energy to algorithms illustrating the difficulties of showing languages are related. So the suggestion that the resemblances leading to a proposal of a relationship are 'chance' appears at first sight persuasive.

But in fact attributing resemblance to 'chance' is a virtually worthless heuristic, because it is an untestable proposition, since no empirical data can ever be adequate to exclude it. Amassing evidence may make any linguistic proposition more likely, but a negative can never be demonstrated. In other words, it can never be shown that the apparent relation between two lexemes is *not* due to chance. Clearly, it is always possible to find unrelated languages where individual items show close sound/meaning correspondences. Our assumption that the languages in question are unrelated is partly determined by geography, partly by the lack of a regular relationship. But the regularity of a relationship can really only be determined by comparative data. If one language shows lookalikes and its genetic relatives do not, borrowing or chance may be the explanation. But if languages have no close relatives, then it is problematic to exclude these alternatives. The calculations by Bender and Ringe assume that languages have no structure, that in principle any combination of CV phonemes may arise. But in practice this is not true, since most languages are extremely constrained in their permissible canonic structures. If two languages are related, then the set of lexemes said to be cognate should have constraints on both phonology and canonic forms.

The assumption of chance is thus an unusable tool. We can draw up tables of more or less likely cognates, and whether these are accepted by other linguists is a function of the credibility of the sound-meaning correspondences and demonstration that these are not borrowings. The reduction of the numbers of phyla in Australian languages is a good example of this. Earlier descriptions supposed there were at least twenty-five Australian language phyla, a figure which has been reduced to ten or eleven as a consequence of a better understanding of historical morphology. This in turn is a function of the growth of descriptive literature and the intensity of research on these languages. African languages are never likely to have the same degree of attention; results come slowly. However, they do come, as witness the characterisation of Omotic (Bender 1975, 2001, 2003) and its gradual acceptance by the Afroasiatic establishment.

4.3 Nilo-Saharan and historical linguistics

The previous section mentions widespread doubts over the affiliation of languages claimed to be Nilo-Saharan. Part of the issue is the way historical linguistics is conventionally conducted. Compared with other African phyla, the difficulties of demonstrating the reality of Nilo-Saharan have typically propelled authors into methodological excursuses (Bender 1997; Ehret 2001; Blench 2002). It seems highly unlikely that Nilo-Saharan will ever pass the tests of regular sound correspondences and an agreed internal structure now part of the formula for the usual textbooks on historical linguistics. In other words, Nilo-Saharan will never look like Austronesian or Dravidian. There are simply not enough undisputed lexical cognates to set up secure correspondences or develop clouds of isoglosses illustrating particular subgrouping hypotheses. For those who really don't want to see Nilo-Saharan there will probably can never be enough evidence to support it.

Greenberg (1987), prescient as ever, was one of the first to point to the problems with textbook historical linguistics. His preferred method, 'mass comparison', was intended to circumvent the issues of poor data and low-level cognacy. However, whether 'mass comparison' could ever be treated as a method rather than as the insights of an exceptional individual is open to doubt. Greenberg's 'success' with African languages is often cited as a reason for accepting his more controversial hypotheses, such as Eurasiatic or Indo-Pacific. But the truth is that Nilo-Saharan is the only one of his African hypotheses that was genuinely new. Niger-Congo, Afroasiatic and Khoisan were all picked up from previous authors (Westermann (1927), Cohen (1947), Bleek (1956)) and neither Amerind nor Eurasiatic have received comparable scholarly assent[11]. Nonetheless, in the grand scheme of things, Greenberg was the first to outline numerous points of lexicon and morphology that remain of importance.

The problem arises because the methods we use for historical linguistics derive from 'neater' phyla. A phylum that has gradually expanded across a terrain (Austronesian, Uralic) and falls into well-defined subgroups is always going to be closer to this ideal structure. But the historical conditions responsible for Nilo-Saharan are quite different. It apparently dispersed across Africa in a period when foraging was still the only method of subsistence. A recent model proposes that this was an east-west expansion and that the key factor was the availability of aquatic resources in Saharan and Sahelian regions (Drake, Blench et al. 2011). Later expansions of unrelated phyla have encapsulated or heavily influenced each of its branches, inducing heavy lexical and structural borrowing and extensive morphophonological shifts. History has often moved on, obscuring these patterns. The consequence is that lexical cognates are few, fragmentary and may now be widely geographically dispersed, to the extent that they can be hard to distinguish from chance.

However, the historical morphology of Nilo-Saharan plays a role in making cognates so difficult to discern. Niger-Congo languages typically have morphological features which characterise most of its branches, most notably nominal class affixes and verb extensions. Although these do not occur everywhere, they are widespread and when lost can often be recognised through fossil morphology. Indeed, as Westermann (1935) pointed out long ago, the detailed similarities between the affixes across widely geographically disparate branches provide useful preliminary evidence for the reality of the phylum.

Nilo-Saharan has no such instantly recognisable features. Some branches have nominal class affix alternation (e.g. Koman, Daju, Kadu) but these do not show alliterative concord and the affixes are not obviously linked with semantics. Languages of the Kadu group are particularly misleading, since at first they seem to have nominal affixes, leading Greenberg (1963) to mistakenly consider 'Tumtum' as part of Kordofanian (Blench 2006). But these are very remote from the systems of Niger-Congo, and form part of a complex three-term system of noun plurals. I have previously argued that Nilo-Saharan must once have had a noun-class system like Niger-Congo and hence the two phyla could be united into a singular macrophylum (Blench 1995). I now consider that the common features of the two phyla can equally well be explained by early borrowing.

---

[11] John Bengtson (p.c.) comments 'The only scientifically valid way to refute Greenberg's Indo-Pacific or Amerind is to show that a different classification is superior'. However, demonstrating negative hypotheses, i.e. that two languages are not related, is not the structural opposite of a positive claim. The specialist may claim that the proposed phonological relationships or semantic shifts are 'unlikely' but we have no gold standard of what is unlikely. Clearly improbable shifts do occur. The linguistic community will inevitably go with the weight of scholarly opinion, in the awareness it may be revised.

Nilo-Saharan has characteristics which make recognising cognates particularly challenging. These are:

a) words erode from the front, i.e. they lose C_1 [usually a morpheme] and sometimes replace it with another consonant [also often a morpheme][12]
b) metathesis of the root is common (which is probably related to a)
c) conservation of unproductive affixes (as many as three on one word) making it difficult to discern the root.

The underlying reason is probably an original numeral classifier system resembling Asian languages, which is heavily eroded or appears synchronically only as frozen affixes. Various studies have noted associations between affixes and semantic themes. For example, both Stevenson (1991) and Gilley (2014) note the semantic associations of affix pairings in Kadu languages, and Storch (2005) remarks on these for Western Nilotic. But the most striking evidence comes from Gumuz, whose Mayu dialect has been studied by Ahland (2010). Gumuz has a system of nominal incorporation, where a series of body part nouns are incorporated into verbs and which 'classify' the object, or more rarely the subject or instrument. Blench (2014) argues that this reflects the original system of Nilo-Saharan, where the predicate classifiers are bound to the verb, or a system of nominal classifiers preceding the noun, which were appended to the verb in Gumuz. Ahland (2010) reports that the neighbouring Bertha language may also have such a system, although this has not been written up. If this is the case, it explains the disconcerting way number markers are copied between prefix and suffix slots in Nilo-Saharan. Gumuz shows clearly that a range of permissible word orders allow the predicate classifier to show up in different places in the sentence.

The typological parallel here is Austroasiatic, a phylum with thirteen branches like Nilo-Saharan, and also dispersed through aquatic movement, in this case along the Mekong some 4000 years ago (Sidwell & Blench 2011). Austroasiatic has a system of roots and single consonant prefixes, although the prefixes have no obvious semantic associations synchronically. However, they can vary alarmingly from one branch of Austroasiatic to another, and to establish cognates it is necessary to ignore them and compare stems. Fortunately, C_1 can be deleted or replaced in Austroasiatic, but the prefix does not become a suffix, nor does the prefix combine with the stem consonant to produce complex consonants, all of which make it possible to recognise cognates more easily.

The methodological point should now be more evident. Some languages at first sight look problematic to relate to others, because of our rather rigid views of historical morphology. There is a sort of psychodrama in linguistics between the 'long-rangers', whose house-journal is *Mother Tongue* and the distinct but related the website 'Tower of Babel' established by Sergei Starostin. Those who see long distance connections between languages are often impatient with the sceptics and some can be seen as riding roughshod over the usual rules of historical linguistics. At the other end of the spectrum are those who can never relate anything to anything else unless rigorous sound-correspondences are in place, represented by Glottolog. Nilo-Saharan is not made for them. We have to tread a line between Voltairean genealogies, where everything is related, and out and out scepticism, accepting that some phyla and macrophyla mean bending the rules.

---

[12] One of the curious aspects of this morpheme replacement is that they appear to have no synchronic meaning in any Nilo-Saharan branch. Greenberg and Bender both assumed they were articles, which makes sense, but cannot be supported from descriptive data.

5. Detecting substrates in animal names

5.1 Madagascar

One of the most interesting scenarios for retention of substrate vocabulary is where an expanding population comes into contact with a new ecology. For example, an inland population reaches the sea-coast or a savanna population enters the equatorial forest. In the most extreme case, an incoming population arrives from an exotic biogeographical zone of the world and encounters a fauna and flora that is almost entirely unfamiliar. This is the case in Madagascar, where Austronesians from island SE Asia, arriving around the 5th century AD, were faced with an largely endemic ensemble of plants and animals and were obliged to construct a new vocabulary to name them. Appendix 1. shows a sample of mammal names in Malagasy with my proposals for their etymologies. A very small proportion are directly from Austronesian, others derive from coastal Bantu and many are quite mysterious. Remarkably, even words for domestic animals such as 'cow' and 'pig' with which the Austronesians were familiar are replaced by Bantu. The likely explanation is that the Austronesians first reached, not Madagascar, but the East African coast. They enslaved local Bantu populations, who were carried to Madagascar to herd cattle and grow rice, and thus became more familiar with the endemic plants and animals than their masters (Blench 2010). A third element in Malagasy animal names are words with no clear provenance. It was previously thought that Madagascar was uninhabited prior to Austronesian settlement, but archaeological evidence suggests that low-density foragers from the African mainland reached the islands around 400 BC (Blench 2007b). Dewar et al. (2013) have recently claimed that stone tools push this date back to earlier than 2000 BC, although it is safe to say, that this is not yet widely accepted. There is presumably a link with the still-extant foragers, the Bēosi or Vazimba, whose language is now only recoverable as isolated lexicon from fragments recorded in the 1930s (Birkeli 1936; Blench & Walsh n.d.; Pierron et al. 2014). It is reasonable to suppose that animal names without etymologies in Malagasy are substrate elements from the prior languages of foragers who reached the island before the ancestors of the present-day occupants.

5.2 Fishing populations of the West African coast

The Ịjọ peoples, who today inhabit the Niger Delta in Nigeria, are presumed to have migrated from the Upper Niger several thousand years ago[13]. Some 3000 years ago there is a major increase in oil-palm pollen in the Delta (Ṣowunmi 1981) which probably points to an incoming population cutting down gallery forest. Whether the Ịjọ displaced an indigenous foraging population has not been resolved, but their ichthyological vocabulary is quite consistent, and yet shows no etymological affinities with either the names of river fish, nor signs of being constructed from language-internal resources. Appendix Table II gives a list of the principal fish names that can be reconstructed to proto-Ịjọ. Many vernacular terms for sea-fish names seem to appear from nowhere. This suggests strongly that there were pre-existing maritime populations specialised in catching pelagic species, and that the incoming Ịjọ adopted fish-names from them. Once these populations were assimilated, they persist in the vocabulary of sea-creatures.

5.3 Animal names of the ancient Sahara

Archaeological evidence points to an extremely long occupation of the Sahara (Drake, Blench et al. 2011), yet the languages spoken there today are clearly of recent origin. The Arabic-speaking Hassaniya that dominate Mauretania have all but assimilated the Zenaga, the last remaining Berber group (Taine-Cheikh 2010). The Central Sahara is virtually all Tuareg-speaking apart from the small island of Tetseret Berber in Niger (Lux 2011b). The Saharan languages, Teda-Daza and Beria [=Zaghawa], occupy the Chad-Sudan borderland and the eastern Sahara is again Arabic-speaking Bedawiin. Berber languages are extremely close to one another and cannot be very ancient. Blench (2001) identifies the spread of Berber languages with the east-west spread of ruminant livestock, principally cattle and sheep, which would put the Berber populations in Mauretania by about 4300 BP.

There are some populations in the Sahara which may be remnants of older foraging groups. The Imraguen, fishermen living along the sea-coast of Mauretania, the Nemadi migrating between Mali and eastern Mauretania and the Dawada, a now-dispersed people who subsisted on the endemic shrimps in the salt lakes of southern Libya. None of these peoples now speak a distinctive language, and their Arabic has not been systematically elicited to establish whether they retain specialised technical vocabulary which cannot be effectively etymologised.

Populations such as the Tuareg, moving from the oases of North Africa south into the desert and into the Sahel of West Africa would encounter unfamiliar plants and animals. They seem to have partly taken over names from the prior Berber populations, but some have no clear etymologies and they probably adapted these from the speech of forager populations the resident in the Sahara. Appendix III gives some examples of this lexicon.

---

[13] One striking piece of evidence for this is the cognacy of the Ịjọ word for 'manatee' (*Trichechus senegalensis*) with the Mande terms, Proto-Ịjọ *imẽĩ* and Bamana *mã̌*.

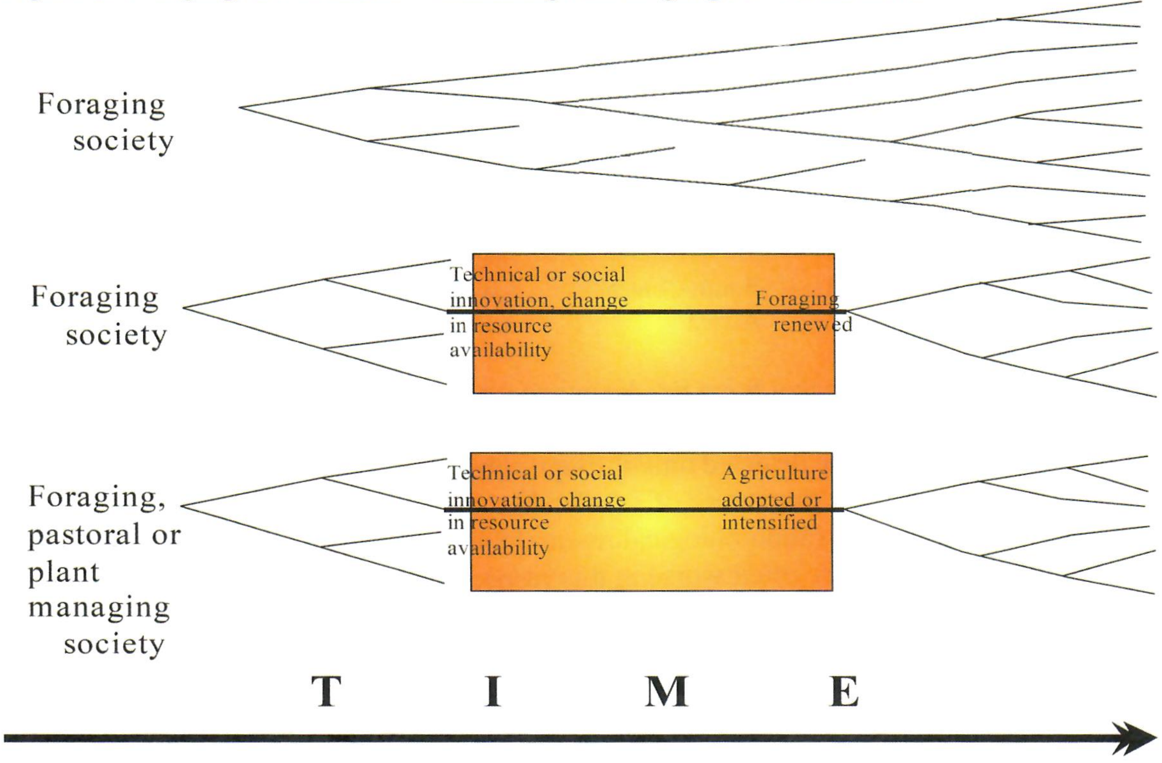
6. Conclusion: accounting for the linguistic situation in Africa

We cannot easily dispense with a stage in African prehistory when the continent was characterised by extreme linguistic and biological diversity. At the period when modern humans were diffusing from southern and eastern Africa, they would have spread over the continent at extremely low population densities either assimilating or out-competing existing hominid populations. Modern humans must have been interfertile with some resident African hominids, creating biological, social and linguistic complexity. Whether there was a pause before they entered West Africa remains a topic for further research. Most of these populations would have been physically unlike the Khoisanoids and resembled rather the Hadza, the Ongota, the Kwadi and the Damara (Blench 1999). If so, then this earlier diversity has largely disappeared and this must be accounted for by expansions of major language phyla. Such expansions are not unmotivated; there must be some economic or social innovation to account for them and the challenge is to trawl the archaeological and linguistic record for possible motivating factors. Ideally, the model would also explain the major differences between global language areas. The argument can be summarised as follows:

a) Language goes back into the unknown past of foraging societies and probably to the genesis of modern humans. The default behaviour of such societies is to slowly expand demographically, and their languages to eventually diversify to a point where individual speech-forms are no longer relatable to one another.
b) A major 'punctuation' occurs when there is a change in resource availability, and the technical or social capacity to exploit that resource. These factors underlying these changes can be external, such as climate change, or internal, such as religious innovation, the invention of the outrigger or the bow and arrow.
c) Such changes provide a significant impetus to particular ethnolinguistic groups and they expand geographically, either demographically or through assimilation. This process is *more effective* among foragers than among cultivators but may result in fewer languages. Agriculture can have the effect of slowing down language distantiation, although there may be greater overall numbers of languages due to the increase in speakers.
d) The consequence is a pattern of geographically extensive language phyla dispersed among isolates or small phyla. Such extensive phyla can be fragmented or coherent, depending on the nature of the impulsion.
e) Gradual intensification of plant or animal management, to the point where it can be defined as agriculture, may therefore occur when the expanding phylum encounters a resource bottleneck. Where there is no bottleneck, foraging continues, and where the resources/demography equation favours foraging, devolution back to foraging can occur.

Figure 2 presents these alternative processes as a diagram:

Figure 2. Foraging, the transition to farming and language diversification


Foraging society
Foraging society
Technical or social innovation, change in resource availability
Foraging renewed
Foraging, pastoral or plant managing society
Technical or social innovation, change in resource availability
Agriculture adopted or intensified
T I M E

Since the beginning of the Holocene there have been a series of phylic expansions within Africa, which have effectively eliminated its linguistic diversity. Fragmentary evidence for this remains in the few isolates still extant, as well as substrate lexicon. The analysis of substrate lexicon in Africa remains poorly developed, probably due to the intellectual tradition of 'lumping', although it is relatively easy to detect, especially in the lexicon of plants and animals.

Table 4 shows the phyla that now dominate Africa with my proposals for their origins, approximate dates and engines of dispersal (Blench 2006b). This proposal does not cover all of Khoisan, only the central Khoe languages, which are all closely related (Voßen 1996). The fragmentary nature of our documentation on other Khoisan branches may mean that these questions are in principle unanswerable.

Table 4. African language phyla: dates, homelands, archaeological correlations

Phylum	**Date BP**	**Homeland**	**Engine**	**Correlation**
Nilo-Saharan	>15,000	SW Ethiopia, Uganda	Climate improvements, fishing	'Green Sahara'
Niger-Congo	>10,000	Southern margins of the Sahara	New hunting techniques	Ounanian?
Afroasiatic	>10,000	SW Ethiopia	Livestock management	
Central Khoisan	>2000	South Central Africa	Livestock management	

The consequence has been that *bona fide* language isolates in Africa are very rare, and their status often debated. They must have been extremely common at some point, but have been assimilated and now can be detected only through substrate vocabulary. It seems likely that this is quite common, but the nature of scholarship applied to African languages has not been focused on this area. Ethnobiological research has declined drastically in recent years and modern dictionaries are often worse in this respect than those compiled half a century ago, probably because typical collaborators are semi-urbanised and now have no knowledge of the natural world. In addition, environmental degradation has meant that even those living in rural areas are unfamiliar with many species of plant and animal which would have been well-known a few decades ago. Just as linguists are much better at holding conferences deploring the disappearance of languages rather than going out and documenting them, so the same may also be true of research into the lexicon of the natural world and the detection of substrates.

As to the larger picture, we are still at the level of speculation. The situation in Africa may be said to resemble Eurasia, including SE Asia, where the expansion of a few phyla has almost completely eliminated diversity, leaving only scattered isolates and small phyla. This can be contrasted with Melanesia, Australia and the New World, where a greater diversity is the overall pattern, despite Holocene expansions of Pama-Nyungan and the Trans New Guinea phylum. It is noticeable that these three areas represent the 'end-of-the-line' as far as expansion of modern humans goes. They are geographically remote and thus were relatively isolated from technical and resource innovation, which drove language expansion. The situation in Africa and Eurasia seems to be that a large contiguous land area provides a breeding ground for innovation and crucially, its spread, thereby increasing the opportunities for innovation to be adopted and transform individual small forager societies. Hence the rather remarkable differences between regions of the world.

References

Abbi, Anvita 2006. *Endangered Languages of the Andaman islands*. Munich: Lincom Europa.

Abbi, Anvita 2012. *Dictionary of the Great Andamanese Language (English – Great Andamanese – Hindi)*. (First Edition with a CD-ROM). Delhi, India: Ratna Sagar.

Ahland, C.A. 2010. Noun Incorporation and Predicate Classifiers in Gumuz. *Journal of African Languages and Linguistics*, 31(2):159-203.

Anonby, Erik 2007. The labial flap in Mambay (Niger-Congo, Adamawa): Phonological rarity or fundamental element? *Journal of African languages and linguistics* 28:1, 1–17.

Bar-Yosef, O. & Belfer-Cohen, A. 2001. From Africa to Eurasia-early dispersals. *Quaternary International,* 75: 19-28.

Beekes, Robert S.P. 2010. The Pre-Greek Loanwords in Greek. In: *Etymological Dictionary of Greek*. Leiden: Brill.

Behar, Doron M Richard Villems, Himla Soodyall, Jason Blue-Smith, Luisa Pereira, Ene Metspalu, Rosaria Scozzari, Heeran Makkan, Shay Tzur, David Comas, Jaume Bertranpetit, Lluis Quintana-Murci, Chris Tyler-Smith, R. Spencer Wells and Saharon Rosset 2008. The dawn of human matrilineal diversity. *American Journal of Human Genetics*, 82(5) : 1130-1140.

Bender, M. L. 1975. *Omotic: A New Afroasiatic Language Family*. University Museum, Southern Illinois University, Carbondale.

Bender, M. L. 1979. Gumuz: a sketch of grammar and lexicon. *Afrika und Übersee*, 62:38-69.

Bender, M.L. 1969. Chance CVC correspondences in unrelated languages. *Language*, 45(3): 519-531.

Bender, M.L. 1997. [2nd ed.] *The Nilo-Saharan languages: a comparative essay*. Munich: Lincom Europa.

Bender, M.L. 2001. *Comparative Morphology of the Omotic Languages*. LINCOM Studies in African Linguistics 19. München: Lincom Europa.

Bender, M.L. 2003. *Comparative Omotic Lexicon*. Carbondale: SIU.

Bender, M.L.1983. Remnant languages of Ethiopia and Sudan. in Bender, M.L. (ed.) *Nilo-Saharan Language Studies*. Michigan State University Press.

Birkeli, Emil 1936. *Les Vazimba de la cote ouest de Madagascar : notes d'ethnologie*. Mémoires de l'Académie malgache. Tananarive : Imprimerie moderne de l'Emyrne, Pitot de la Beaujardière.

Bleek, Dorothea 1956. *A Bushman dictionary*. New Haven: American Oriental Society.

Bleek, Wilhelm H.I. 1862, 1869. *A comparative grammar of South African Languages.* (1862: Part I; 1869: Part II) London, Trübner & Co.

Blench, Roger M. 1995. Is Niger-Congo simply a branch of Nilo-Saharan? In: *Proceedings of the Fifth Nilo-Saharan Linguistics Colloquium, Nice, 1992*. ed. R. Nicolai and F. Rottland. 68-118. Köln: Rudiger Köppe.

Blench, Roger M. 2001. Types of language spread and their archaeological correlates: the example of Berber. *Origini*, XXIII: 169-190.

Blench, Roger M. 1999. Are the African pygmies an ethnographic fiction? In: *Hunter-gatherers of Equatorial Africa*. K. Biesbrouyck, G. Rossel and S. Elders (eds.) 41-60. Leiden: Centre for Non-Western Studies.

Blench, Roger M. 2002. Besprechungsartikel. The classification of Nilo-Saharan. *Afrika und Übersee*, 83:293-307.

Blench, Roger M. 2006a. *The Kadu languages and their affiliation: between Nilo-Saharan, Niger-Congo and Afro-Asiatic*. Insights into Nilo-Saharan Language, History and

Culture. Al-Amin Abu-Manga, L. Gilley & A. Storch eds. 101-127. Köln: Rüdiger Köppe.
Blench, Roger M. 2006b. *Archaeology, Language and the African past*. Lanham: Altamira Press.
Blench, Roger M. 2007a. Bangi Me: a language of unknown affiliation in Northern Mali. *Mother Tongue XII*: 147-178.
Blench, Roger M. 2007b. New palaezoogeographical evidence for the settlement of Madagascar. *Azania* XLII:69-82.
Blench, Roger M. 2010. New evidence for the Austronesian impact on the East African coast. In: *Global origins and the development of seafaring*. Atholl Anderson ed. Cambridge: Macdonald Institute.
Blench, Roger M. 2012. The role of agriculture in explaining the diversity of Amerindian languages. In: *The past ahead: language, culture and identity in the Neotropics.* Christian Isendahl ed. 13-37. Uppsala: Acta Universitas Uppsaliensis.
Blench, Roger M. 2013. Links between Cushitic, Omotic, Chadic and the position of Kujarge. In: *Proceedings of the 5th International Conference of Cushitic and Omotic languages*. Mari-Claude Simeone-Senelle & M. van Hove eds. 67-80. Köln: Rüdiger Köppe.
Blench, Roger M. 2014. The origins of nominal affixes in MSEA languages: convergence, contact and some African parallels. In: *Mainland Southeast Asian Languages: The State of the Art 2014*. N.J. Enfield and Bernard Comrie eds. Mouton de Gruyter.
Blench, Roger M. n.d. a. *Recovering data on Mpra [=Mpre] a possible language isolate in North-Central Ghana*. Electronic ms.
Blench, Roger M. n.d. b. *A Dompo wordlist*. Electronic ms.
Blench, Roger M. n.d. c. *Bɛrɛ: a language of Côte d'Ivoire of unknown affiliation*. Electronic ms.
Blench, Roger M. & M. Walsh n.d. *The vocabularies of Vazimba and Beosi: do they represent the languages of the pre-Austronesian populations of Madagascar*? Unpublished paper.
Boyd, Raymond 1994. *Historical perspectives on Chamba Daka*. Köln: Rudiger Köppe.
Boyeldieu, P. n.d. *Presentation du láà:l ou "Gori" (Moyen-Chari, Tchad).* ms. CNRS, Paris.
Brown, Cecil n.d. *Statistical Demonstration of the Failure of Key Linguistic Evidence to Support the Amerind Hypothesis*. Circulated ms.
Campbell, Lyle 1997a. *American Indian languages: the historical linguistics of native America*. New York/Oxford: Oxford University Press.
Campbell, Lyle 1997b. Amerind personal pronouns: A second opinion. *Language* 73:339-351.
Campbell, Lyle & W.J. Poser 2008. *Language classification*. Cambridge: Cambridge University Press.
Cardinall, A.W. 1931. A survival. *Gold Coast Review*, 5(1):193-197.
Cohen, M. 1947. *Essai comparatif sur le vocabulaire et la phonétique du Chamito-Sémitique*. Paris: Honoré Champion.
Cohen, A.S., Stone, J.R., Beuning, K.R., Park, L.E., Reinthal, P.N., Dettman, D., Scholz, C.A., Johnson, T.C., King, J.W., and Talbot, M.R., et al. 2007. Ecological consequences of early Late Pleistocene megadroughts in tropical Africa. *Proceedings of the National Academy of Sciences of the United States of America,* 104:16422-16427.
Creissels, Denis n.d. *Liste de mots Pre.* ms.
Crowley, T. and R.M.W. Dixon 1981. Tasmanian. In: Handbook *of Australian languages, Volume 2.* R.M.W. Dixon and B.J. Blake (eds). 394-427. Canberra: Australian National University Press.
Dempwolff, Otto 1934. *Vergleichende Lautlehre des austronesischen Wortschatzes, Band 1: Induktiver Aufbau einer indonesischen Ursprache.* Beihefte zur Zeitschrift für Eingeborenen-Sprachen 15. Berlin: Dietrich Reimer.

Dempwolff, Otto 1937. *Vergleichende Lautlehre des austronesischen Wortschatzes, Band 2: Deduktive Anwendung des Urindonesischen auf austronesische Einzelsprachen.* Beihefte zur Zeitschrift für Eingeborenen-Sprachen 17. Berlin: Dietrich Reimer.

Dempwolff, Otto 1938. *Vergleichende Lautlehre des austronesischen Wortschatzes, Band 3: Austronesisches Wörterverzeichnis.* Beihefte zur Zeitschrift für Eingeborenen-Sprachen 19. Berlin: Dietrich Reimer.

Dennell, R. and M. Petraglia 2012. The Dispersal of *Homo sapiens* across Southern Asia: How Early, How Often, How Complex? *Quaternary Science Reviews*, 47:15-22.

Dewar, R.E., Radimilahy, C., Wright, H. T., Jacobs, Z., Kelly, G. O., & Berna, F. 2013. Stone tools and foraging in northern Madagascar challenge Holocene extinction models. *Proceedings of the National Academy of Sciences*, 110(31): 12583-12588.

Dimmendaal, Gerrit J. 2011. *Historical linguistics and the comparative study of African languages*. Amsterdam: John Benjamins Publishing.

Dixon, R.M.W. 1997. *The rise and fall of languages.* Cambridge: Cambridge University Press.

Doke, C.M. 1961. The earliest records of Bantu. In: *Contributions to the history of Bantu linguistics*. C.M. Doke and D.T. Cole eds. 1-26. Johannesburg: Witwatersrand University Press.

Doornbos, P. & M.L. Bender 1983. Languages of Wadai-Darfur. In: *Nilo-Saharan Language Studies*. M.L.Bender (ed.) 43-79. East Lansing: Michigan State University Press.

Drake, N.A., Blench, Roger M., Armitage, S.J., Bristow, C.S., White K.H. 2011. Ancient watercourses and biogeography of the Sahara explain the peopling of the desert. *Proceedings of the National Academy of Sciences of America*, 108, 458-462.

Ehret, Christopher 2001. *A historical-comparative reconstruction of Nilo-Saharan*. Köln: Rudiger Köppe.

Fleming, Harold C. 1991. Shabo: presentation of data and preliminary classification. In: *Proceedings of the Fourth Nilo-Saharan Conference. Bayreuth, 1989*. Bender, M L. (ed.) 389-402. Hamburg: Buske.

Fleming, Harold C. 2006. *Ongota: a decisive language in African prehistory*. Mainz: Harassowitz.

Gilley, Leoma 2014. Katcha noun morphology. In: *New research in the languages of the Nuba mountains. Nuba mountains language studies*. Thilo Schadeberg & Roger Blench eds. 501-521. Köln: Rudiger Köppe.

Greenberg, J.H. & Merritt Ruhlen 2007. *An Amerind etymological dictionary*. Department of of Anthropological sciences, Stanford University.

Greenberg, J.H. 1963. *The Languages of Africa*. The Hague: Mouton.

Greenberg, J.H. 1971. The Indo-Pacific hypothesis. In: *Current Trends in Linguistics, Volume VIII - Linguistics in Oceania*, T.A. Sebeok (ed.) 807-871. The Hague: Mouton.

Greenberg, J.H. 1987. *Language in the Americas*. Stanford, CA: Stanford University Press.

Greenberg, J.H. 1996. In defense of Amerind. *International journal of American linguistics*, 62(2): 131-164.

Greenberg, J.H. 2000. *Indo-European and its closest relatives: the Eurasiatic language family*. Stanford, CA: Stanford University Press.

Grün, R., Stringer, C., McDermott, F., Nathan, R., Porat. N., Robertson, S. et al. 2005. U-series and ESR analyses of bones and teeth relating to the human burials from Skhul. *Journal of Human Evolution*, 49: 316–34.

Güldemann, Tom 2004. Reconstruction through 'de-construction': the marking of person, gender and number in the Khoe family and Kwadi. *Diachronica*, 21(2):251-306.

Güldemann, Tom 2008. The Macro-Sudan belt: towards identifying a linguistic area in northern sub-Saharan Africa. In: Bernd Heine & Derek Nurse (eds), *A linguistic geography of Africa*. 151-185. Cambridge: Cambridge University Press.

Güldemann, Tom 2011. Proto-Bantu and Proto-Niger-Congo: Macro-areal typology and linguistic reconstruction. In: *Geographical typology and Linguistic areas.* O. Hieda, C. König and H. Nakagawa eds. 109-142. Amsterdam/Philadelphia: John Benjamins.

Gunz, P., Bookstein, F.L., Mitteroecker, P., Stadlmayr, A., Seidler, H. & Weber, G.W. 2009. Early modern human diversity suggests subdivided population structure and a complex out-of-Africa scenario. *Proceedings of the National Academy of Sciences of America*, 106:6094-6098.

Hall, Ed and Marian Hall 2004. Kadugli-Krongo. *Occasional Papers in the Study of Sudanese Languages*, No. 9. Khartoum.

Hammarström, H. 2012. Pronouns and the (Preliminary) Classification of Papuan languages. *Language and linguistics in Melanesia*, Special issue 2012, 2: 428-539.

Heath, Jeffrey 2006. *Dictionnaire touareg du Mali*. Paris: Karthala.

Heine, Bernd & Derek Nurse (eds.) 2008. *A linguistic geography of Africa*. Cambridge: Cambridge University Press.

Heubeck, Alfred 1961. *Praegraeca: sprachliche Untersuchungen zum vorgriechisch-indogermanischen Substrat.* Universitätsbund Erlangen.

Hyman, L.M. 2011. The Macro-Sudan Belt and Niger-Congo Reconstruction. *Language Dynamics and Change*, 1(1):3-49.

Jackson, K. 1955. The Pictish Language. In: Wainwright, F.T. ed. *The Problem of the Picts*. 129–166. Edinburgh: Nelson.

Ke, Yuehai *et al.* 2001. African Origin of Modern Humans in East Asia: A Tale of 12,000 Y Chromosomes. *Science*, 292: 1151-1153.

Kleinewillinghöfer, U. 2001. The language of the Jalaa: a disappearing language isolate. *Sprache und Geschichte in Afrika*, 16/17: 239-271.

Lewis, P. et al. 2013. *Ethnologue Language Family Index (17th edition)*, Dallas: Summer Institute of Linguistics.

Lionnet, F. (n.d.) *Laal: an isolate language*? Powerpoint presented in Lyon, November 2010.

Lockwood, W.B. 1975. *Languages of the British Isles, Past and Present*. London: André Deutsch.

Lovestrand, J. 2012. *Classification and description of the Chadic languages of the Guéra (East Chadic B)*. SIL Electronic Working Papers 2012-004. Dallas, Texas: SIL International.

Lux, Cécile 2011. *Étude descriptive et comparative d'une langue menacée : le tetserret, langue berbère du Niger*. Ph.D. Université Lumière Lyon 2 École doctoral : Lettres, langues, linguistique, arts.

Macaulay, Vincent *et al.* 2005. Single, rapid coastal settlement of Asia revealed by analysis of complete mitochondrial genomes. *Science*, 308:1034-1036.

MacDougal, Ian, Francis H Brown, & John G. Fleagle 2005. Stratigraphic placement and age of modern humans from Kibish, Ethiopia. *Nature,* 433: 733-736.

Masica, C.P. 1993. *The Indo-Aryan Languages*. Cambridge: Cambridge University Press.

Mfonyam, Joseph 1989. *Tone in Orthography: The Case of Bafut and Related Languages*. PhD Thesis. University of Yaoundé 1.

Olson, Kenneth S. and John Hajek 2003. Crosslinguistic insights on the labial flap. *Linguistic Typology,* 7,2: 157-186.

Painter, C. 1967. The distribution of Guang in Ghana and a statistical pre-testing on twenty-five idiolects. *Journal of West African Languages*, 4,1:25-78.

Pawley, Andrew 2005. The chequered career of the Trans New Guinea hypothesis: recent research and its implications. In: *Papuan Pasts: Cultural, linguistic and biological histories of Papuan-speaking peoples*. Andrew Pawley, Robert Attenborough, Jack Golson and Robin Hide (eds.) 67-108. PL 572. Canberra: ANU.

Pawley, Andrew 2012. How reconstructable is proto Trans New Guinea? Problems, progress, prospects. *Language and linguistics in Melanesia*, Special issue 2012, 1: 88-164.

Petraglia, M.D. 2007. Mind the gap: factoring the Arabian peninsula and the Indian subcontinent into Out of Africa models. In: *Rethinking the Human Revolution*. P. Mellars, O. Bar-Yosef, K. Boyle & C. Stringer eds. 383-394. Cambridge: McDonald Institute Archaeological Publications.

Petraglia, M.D. and J.I. Rose (eds.) 2009. *The Evolution of Human Populations in Arabia*. Dordrecht: Springer Verlag.

Petraglia, M.D., Haslam, M., Fuller, D.Q. & Boivin, N. 2010. The southern dispersal route and the spread of modern humans along the Indian Ocean rim: New hypotheses and evidence, *Annals of Human Biology*, 37(3): 288-311.

Pierron, Denis, Harilanto Razafindrazaka, Luca Pagani, François-Xavier Ricaut, Tiago Antao, Mélanie Capredon, Clément Sambo, Chantal Radimilahy, Jean-Aimé Rakotoarisoa, Roger M. Blench, Thierry Letellier, Toomas Kivisild 2014. Genome-wide evidence of Austronesian–Bantu admixture and cultural reversion in a hunter-gatherer group of Madagascar. *Proceedings of the National Academy of Sciences of the United States of America*, http://www.pnas.org/content/early/2014/01/03/1321860111.abstract

Rankin, Robert L. 2012. Deeper Genetic Relationships in North America: Some Tempered Pessimism. In: *Proceedings of the Annual Meeting of the Berkeley Linguistics Society*, 22(2): 117-128.

Rankin, Robert L.1992. Review: Joseph H. Greenberg, Language in the Americas. *International Journal of American Linguistics*, 58:324-351

Ringe, Don A. 1992. On calculating the factor of chance in language comparison. *Transactions of the American Philosophical Society*, 82(1):1-110.

Ringe, Don A. 1999. Language classification: scientific and unscientific methods. In: *The human inheritance: genes, language, and evolution*. Brian D. Sykes ed. 45-74. Oxford: Oxford University Press.

Rose, J. I., Usik, V. I., Marks, A. E., Hilbert, Y. H., Galletti, C. S., Parton, A., ... & Roberts, R. G. 2011. The Nubian complex of Dhofar, Oman: an African middle stone age industry in southern Arabia. *PloS* 1, 6(11): e28239.

Ruhlen, Merritt 1994. Amerind T'A?NA 'Child, Sibling. In: *On the Origin of Languages*. M. Ruhlen 183–206. Stanford University Press.

Ruhlen, Merritt 1995. Proto-Amerind Numerals. *Anthropological Science* (Tokyo) 103:209–25.

Sands, B. 1998. *Eastern and Southern African Khoisan: evaluating claims of a distant linguistic relationships*. Quellen zur Khoisan-Forschung 14. Köln: Rüdiger Köppe.

Savà, Graziano and Mauro Tosco 2000. A sketch of Ongota: a dying language of Southwest Ethiopia. *Studies in African Linguistics*, 29:2: 59-135.

Sidwell, Paul & R.M. Blench 2011. The Austroasiatic *Urheimat* : the Southeastern Riverine Hypothesis. In: N. Enfield ed. *Dynamics of Human Diversity in Mainland SE Asia*. Canberra: Pacific Linguistics.

Sommer, Gabriele and Rainer Voßen 1992. Schnalzwörter im Yei (R.41). *Afrika und Übersee*, 75:1-42.

Ṣowunmi, M.A. 1981. Late Quaternary Environmental Changes in Nigeria. *Pollen et Spores*, 13(l):125-148.

Stevenson, Roland C. 1991. Relationship of Kadugli-Krongo to Nilo-Saharan: Morphology and lexis. In: *Proceedings of the Fourth Nilo-Saharan Conference. Bayreuth, 1989*. Bender, M L. (ed.) 347-369. Hamburg: Buske.
Storch, Anne 2005. *The Noun Morphology of Western Nilotic*. Köln: Köppe.
Sudlow, David 2009. *Dictionary of the Tamasheq of North-East Burkina Faso.* Köln: Rudiger Köppe.
Sweetman, Gary 1981a. *Lexique francais-fali-fulfulde*. ms. Yaoundé: SIL.
Sweetman, Gary 1981b. *A comparative stud of fali dialects*. ms. Yaoundé: SIL.
Taine-Cheikh, C. 2010. *Dictionnaire français-zénaga (berbère de Maurétanie)*. Köln: Rudiger Köppe.
Trombetti, Alfredo 1905. *L'unità d'origine del linguaggio*. Bologna: Libreria Treves di Luigi Beltrami.
Van Peer, P. 1998. The Nile Corridor and the Out-of-Africa Model: An Examination of the Archaeological Record. *Current Anthropology*, 39:115-140.
Vermeersch, P.M. 2001. Out of Africa' from an Egyptian point of view. *Quaternaire International* 75: 103-112.
Voßen, R. 1996. *Die Khoe-sprachen*. Köln: Rudiger Köppe.
Westermann, Diedrich 1911. *Die Sudansprachen*. Hamburg: Friederichsen.
Westermann, Diedrich 1927. *Die westlichen Sudansprachen und ihre Beziehungen zum Bantu*. Berlin: de Gruyter.
Westermann, Diedrich 1935. Nominalklassen in westafrikanischen Klassensprachen und in Bantusprachen. MSOS, 38: 1-52.
Westphal, E.O.J. 1963. The linguistic prehistory of Southern Africa: Bush, Kwadi, Hottentot and Bantu linguistic relationships. *Africa*, 33: 237-265.
Whitehouse, P., Usher, T., Ruhlen, M., & Wang, W.S.Y. 2004. Kusunda: An Indo-Pacific language in Nepal. *Proceedings of the National Academy of Sciences of the United States of America*, 101(15): 5692-5695.
Wilson, John G. 1970. Preliminary observations on the Oropom people of Karamoja, their ethnic status, culture and postulated relation to the peoples of the late stone age. *Uganda Journal*, 34(2):125-145.

Appendices

I. Malagasy mammal names

No.	Malagasy	English	Scientific name	Etymological commentary
1.	amboa laolo	Falanouc	*Eupleres goudotii*	cf. **amboahaolo** 'feral dog' < **amboa** 'dog' **haolo** 'wild' [R]. **amboa** is from a Bantu source, e.g. Comorian & Swahili **mbwa** (9/10) 'dog' < Proto-Sabaki ***(i)mbwa** (9/10) 'dog' [N&H]. The transfer from 'dog' to 'falanouc' (an endemic carnivore) is perhaps a secondary derivation in Malagasy
2.	ampongy	Eastern avahi	*Avahi laniger*	cf. Swahili (Unguja) **k^{h}ima punju** (9/10) 'Zanzibar red colobus, *Colobus kirkii*'; also Nyakyusa **kipunji** (7/8) 'Highland mangabey, *Rungwecebus kipunji*' [Davenport et al.]. The Malagasy term for this medium-sized lemur may be derived from a form of the Bantu monkey name with class 3 prefix (?***mpungi**)
3.	andrehy [G&B]	Fruit bats	Pteropodidae	Richardson (1885: 43) defines this as the name of a bird. cf. Comorian (Ndzuani) **ndrege** (9/10) 'bird (generic)'; Swahili **ndege** (9/10) 'bird'. This is an innovation in the southern dialects of Swahili, probably borrowed from one of the mainland Bantu languages [N&H]. In Swahili and related languages bats are often classified as birds
4.	ankomba, komba	Crowned lemur	*Eulemur coronatus*	cf. Swahili (Unguja) **k^{h}omba** (9/10) 'galago spp.' [Walsh] < Proto-Sabaki ***nkomba** (9/10) 'galago' [N&H]. Given the resemblance between these two groups of primates, the transfer of a name from galagos (= bushbabies) to lemurs, which are indigenous to Madagascar, is as natural as was the former English practice of referring to bushbabies as 'lemurs'
5.	antsanga	Bushpig	*Potamochoerus larvatus*	cf. Swahili (Unguja) **kitanga** (7/8) 'solitary male bushpig'? The Malagasy form is possibly derived from an earlier ?***ncanga** (9/10) 'male bushpig' < Proto-Sabaki ***-canga** v 'to wander' [N&H]

No.	Malagasy	English	Scientific name	Etymological commentary
6.	antsangy	rice tenrecs	*Oryzorictes spp.*	cf. Swahili (Tanzanian mainland) **sange** (9/10) 'elephant shrew spp.' [Swynnerton]; Mijikenda (Giryama) **tsanje** (?**tsʰanje**) (9/10) 'Four-toed elephant shrew, *Petrodomus tetradactylus*' [Costich] < earlier ?***ntsange**. This term has widespread cognates in Tanzania. Elephant shrews are superficially similar to the endemic rice tenrecs of Madagascar
7.	boenga, boengy	Milne-Edward's sportive lemur	*Lepilemur edwardsi*	cf. Sungai (East Sabah) **bongan** 'Hose's langur'
8.	falanouc	Falanouc	*Eupleres goudotii*	cf. Barito lects also Lun Dayeh (Sabah) **pəlanuk** 'mouse-deer spp.' (*Tragulus napu, T. javanicus*). A strange semantic shift but the form is very close. However, both the size and posture of these two species are not dissimilar
9.	fanihy	Fruit bats	Pteropodidae	cf. PMP ***paniki** 'flying fox'. Blust (2002: 107) notes that reflexes of this are *absent* in Borneo and thus the reflex in Malagasy is rather surprising [see Adelaar on other sources for Malagasy]
10.	fosa	Fossa	*Cryptoprocta ferox*	Beaujard derives this from purported < PMP 'cat' but as Blust (2002: 99) points out, Western Austronesian forms such as Iban **posa** are almost certainly derived from **poes** and these are convergent borrowings from the 17th century. The Malagasy term may therefore be a late and independent borrowing from a trade language. However, it turns out that **pusa** and similar are also Malay for the Malay weasel (*Mustela nudipes*) which may have an old anthropic distribution in the region. It therefore may have shifted to 'cat' in island SE Asia and to fossa in Madagascar. Cf. Malagasy **bosy** 'feral cat' [R] < Swahili **busi** [not in dictionaries] < Arabic [Simon]; also Nyakyusa **pusí** (1a/2) 'cat' [Felberg]
11.	gidro	Crowned lemur	*Eulemur coronatus*	Richardson (1885: 213) suggests a comparison with Swahili **ngedere**

No.	Malagasy	English	Scientific name	Etymological commentary
				'monkey sp.' and/or Arabic **qird** 'ape'. Simon (1988: 291) supports an "araboswahili" etymology. **ngedere** (9/10) is a southern Swahili dialect name for the Blue monkey, *Cercopithecus mitis*, presumed to be borrowed from a neighbouring Bantu language [N&H]. If corroborated this would represent another example of a monkey name transferred to a lemur
12.	kelora	Common tenrec	*Tenrec ecaudatus*	? PCEMP ***kandoRa** 'cuscus, phalanger', e.g. Watubela **kadola**. cf. Blust (2002: 110) though these are east of the usual sources of Malagasy
13.	lambo	Bush pig	*Potamochoerus larvatus*	< Malay **ləmbu**, bovine, the original meaning, surviving in special expressions [Bj]
14.	radjako rajako [Simon] jakoe, jakoey [Gn]	Perrier's sifaka	*Propithecus diadema perrieri*	< French **jacquot** (*faire le jacques, faire le singe*), an abusive term for lemurs [Simon]. Also Indian Ocean Creole **zako**; Comorian **djakwe** [Gn]
15.	tandraka, trandraka tandeke [R]	Common tenrec	*Tenrec ecaudatus*	? cf. Malay **landak** 'porcupine' (Adelaar 1989) and secondary borrowing into Comorian Ngazidja **landa** Ndzuwani **landrá,** Maore **landra** (9/10) 'tenrec'
16.	tranga lavaka	Small-toothed sportive lemur	*Lepilemur microdon*	cf. Kadazan (Sabah) **tangah** 'flying lemur'.
17.	varika	lemur spp.		< Maanyan **warik** 'monkey sp.' [Bj < Dahl]
18.	voalavo, valàvo valave [R]	rat spp.		cf. Proto-Austronesian ***labaw**, e.g. Kayan **lavo**, Muna (Sulawesi) **wulawo**. N.B. there are apparently no reflexes of the common SE Borneo ***lésu** (Blust 2002: 107).
19.	vontsira	Ring-tailed mongoose	*Galidia elegans*	cf. Swahili (Unguja) (9/10) **nguchiro** 'Banded mongoose, *Mungos mungo*' (an introduced sp.) [Pakenham]; probably a loanword from a Rufiji-Ruvuma language, cf. Ngindo **lingwichiro** 'Banded mongoose, *Mungos mungo*'

No.	Malagasy	English	Scientific name	Etymological commentary
				[Stronach et al.]; Matumbi **ngwicho** 'mongoose spp.' [Stronach et al.]; also Pogoro **lingwichiro** 'Dwarf mongoose, *Helogale parvula*, & Banded mongoose, *Mungos mungo*' [Stronach et al.]

1. II. Proto-Ịjọ fish names without discernible etymologies

English	Latin	proto-Ịjọ
Angelfish	*Pomacanthus paru*	ọfụ́n
Barracuda	*Sphyraena spp.*	ḍọrọ
Cameroon Sardine, Shad		bala
Cassava Croaker	*Pseudotolithus senegalensis*	ọpụ́la
Catfish	*Arius* spp.	sịngị, ụngụlụ́
Conger Eel	? *Paraconger notialis*	ábọnị́yọn
Flying Fish	*Cheilopogon sp.*,	mindi-ófoni
Globefish	*Ephippion guttifer*	ububu
Grunt, burro	*Plectorhynchus spp. & Parapristipoma spp.*	ẹgẹlẹ́ụ
Hammerhead Shark	*Sphyrna* spp.	ugubéri
Horse Mackerel	*Caranx spp.*	kidoghó
Ladyfish	*Elops senegalensis*	bala-dowoin
Little Sleeper	*Eleotridae*	kalá ukuli
Mangrove Sleeper	*Eleotridae* sp.	ikúli
Mudskipper	*Periophthalmus spp.*	atị́laị
Pigfish	? *Pomadasys rogerii*	osisi
Ribbonfish	*Trachypterus trachypterus*	bini ọ́pịya, gbɔ́gɔ́
Royal Threadfin	*Pentanemus quinquarius*	ịnda
Sawfish	*Pristis spp.*	oki
Slender Long-Nosed Shark	? *Carcharinus signatus*	ọfụrụma
Snapper	*Lutjanus* spp.	agbará, tọmị
Soapfish	*Rypticus saponaceus*	erému
Sole		bumọpalị
Spanish Mackerel	*Scomberomorus tritor*	sọnọma-siko
Spotted Eagle-Ray ?	*Aetobatus narinari*	ẹgbẹịn
Sting Ray	*Dasyatis spp.*	sịka
Tarpon	*Megalops atlanticus*	ịmụnụ́ dowoi
Tilapia		tabalá
Toadfish	*Batrachoididae* spp.	binị ọ́gụmụ
Turbot		saị́du
Weakfish	*Atractoscion aequidens*	ọna
Jellyfish		álapá
Common Periwinkle	*Tympanotonus fuscatus*	ịsẹmbị́
Dogwhelk	*Nucella lapillus*	igbekete
Oyster Spat	*Ostrea spp.*	ímgbe
Cuttlefish, Squid		burumizi

III. Large animals names in Southern Berber languages

Many of the animal species the Berber encountered as they expanded south into Sub-Saharan Africa were unfamiliar in the Maghreb. The names for these are extracted from the relevant dictionaries. Very few have any obvious etymologies and the assumption is that these were adapted from now-vanished forager populations.

Tetserret animal names

Table 5. Animal names in Tetserret of Niger

English	French	sg.	pl.	Comment
antelope	antilope	ænarˁ		? Dorcas gazelle. Common South Berber
cat, wild	chat sauvage	mzuru		? < Hausa
chameleon	caméléon	tawit		
crow	corbeau		ɔɣrut-ən	
gazelle	gazelle	aʒonkəḍ		
giraffe	girafe	(ə)ʃiyeg	ʃiyeg-ən	
hare	lapin, lièvre	tmarwult, t-əmarwəl-t		
hawk	épervier, aigle	ənollam		
hedgehog	hérisson	tarangat		
hyena	hyène	tafagant		also the name for someone with strange habits (without female markers)
jackal	chacal	eridel		
lion	lion	ar	arr-ən	cf. Zenaga
ostrich	autruche	arəg	argan	
sheep, wild	mouflon	arig	argan	
vulture	vautour, charognard	abəngadew		

Zenaga animal names in Taine-Cheikh (2010)

Table 6. Animal names in Zenaga of Mauretania

English	French	sg.	pl.	Hassaniya
aardvark	oryctérope	təkši-n-tutfən		'*ovin-caprin des fourmis*'
bustard	outarde	ägäyš	əgäyššän	
crow	corbeau	täyyäL	täyyäyən	
dama gazelle	gazelle dama	änaˀr	änaˀrän	cf. Tamachek
dorcas gazelle	gazelle dorcas	äž̠änkuđ	äž̠änkuđ	*Gazella dorcas*. ġzāl
eagle	aigle	aˁdʸi	āˁdʸūn	
elephant	éléphant	iyih	iyān	
francolin	perdrix	tuḟḟurđah	tḟurđäyn	

English	French	sg.	pl.	Hassaniya
gazelle, red-fronted	gazelle à front rouge	až̠əmmi	iž̠əmmaˀn	*Gazella rufifrons* dāmi
giraffe	girafe	aẓ̄ṛaf	əẓ̄ṛaffän	cf. Arabic
hare I	lièvre I	tärämbuL	trumbäyən	
hare II	lièvre II	tyəṛẓ̄aẒ		
hawk	épervier	tuṃdäh	tuṃdäyn	
hedgehog	hérisson	gänvud	əgnāvīd	? < H.
hyena, spotted	hyène, tachetée	gähūh		*Crocuta crocuta.* đ̣abˤ
hyena, striped	hyène, rayé	ärđ̣äy	ərđ̣äyän	
jackal	chacal	äz̄đīh	äz̄đāđän	
leopard	panthère	aġayniš	əġayniššän	
lion	lion	waˀr	waˀrän	cf. Tetserret
monitor lizard	varan	kuđih		
monkey	singe	äbugäy	əbugäyän	
oryx	oryx	wərg		< H.
pelican	pélican	ädänāy		< H.
pigeon	pigeon	iˀmilli	aˀmällän	
python	python	giriž̠ṃä		? < H.
rat	rat	əmīđ̣niš	əmäđ̣näššän	
ratel	ratel	äməssäf ən ürž̠än		'dechireur de tendons d'Achille'
roan antelope	hippotrague	äʒäṃiy	iʒäṃäyän	äʒäməl. cf. Mali Tamachek
sheep, wild	mouflon	äräwih	əräwän	
viper	vipère	täššuffäh	täššuffäyn	
vulture	vautour, 'aigle'	ägoˀđər	əgoˀđərän	cf. Tamachek
warthog	phacochère	aˀž(ž)iy-ən	tnäyriʰ	'âne de la brousse'

Heath's Mali Tamachek dictionary (2006)

Table 7. Animal names in Tamachek of Mali

English	French	sg.	pl.	Comment
aardvark	oryctérope	talæ̈wlæwt		
bat	chauve-souris	a-fæ̀rṭæṭṭa		
buffalo	buffle	ésù		usual word for 'cow'
bustard	outarde	a-s-ækkǽṭṭay		'one who shows his buttocks'
cane rat I	aulacode I	æ-kúndær		but also mouse sp., also dassie
cane rat II	aulacode II	t-e-mḕlu-t-t		but also mouse sp., also dassie
chameleon	caméléon	t-æ̏-haw-t		
cheetah	guépard	á-dhal		
crocodile	crocodile	zəŋgəwáy		also water monitor lizard
crocodile	crocodile	æ̀-ɣata		also land monitor lizard
crow	corbeau	tàkrit		
elephant	éléphant	élu	àlwanæn	

English	**French**	**sg.**	**pl.**	**Comment**
fennec fox	renard	e-žǽgæž		*Fennecus* (= *Vulpes*) *zerda*;
fennec fox	renard	æ-kórsi		
fox, general	renard	e-šǽjæš		
francolin	perdrix	æ-fukǽret		
gazelle, dama	gazelle dama	t-ȅ-nher-t		
gazelle, dorcas	gazelle dorcas	t-ašə̏ŋkəṭṭ		*Gazella dorcas*
gazelle, red-fronted	gazelle à front rouge	e-dǽm	i-dǽman	*Gazella rufifrons*
genet	genet	éltæɣ		
giraffe	girafe	á-mdəɣ		
hare	lièvre	t-e-mæ̏rwæl-t		
hedgehog	hérisson	t-e-kænȅsi-t-t		
hippo	hippopotame	æ̀-jamba		< Songhay
honey badger	ratel	a-fæ̀ẓæẓa		
hooded vulture	vautour	borkíya		
hyena, spotted	hyène, tachetée	šæbójæn		*Crocuta crocuta*
hyena, striped	hyène, rayé	a-rídal		
jackal	chacal	e-bǽgg	ì-bæggan	*Canis adustus*? also *C. aureus*
jerboa	jerboa	e-ḍǽww		*Jaculus jaculus*, also gerbil
leopard	panthère	wàšil		
lion	lion I	á-hærr		
lion	lion II	a-wǽqqas		
monkey	singe	ə-bíddæw		
monkey, patas	singe	kæyá		
oryx	oryx	óræj		*Oryx dammah*, now perhaps extinct locally
oryx	oryx	t-i n ísək		*Oryx dammah*, now perhaps extinct locally
oryx	oryx, topi	t-e-dæ̏ri-t-t		
ostrich	autruche	é-tæqq		
ostrich	autruche	a-nə́hil		
ostrich	autruche	góyba		
pelican	pélican	t-oræ̀f-t n am-an		'x of water'
pigeon	pigeon I	e-dǽber		
pigeon	pigeon II	kəllə́ya		*Oena capensis*
pigeon speckled	pigeon III	t-əzún		
predator	prédateur	bèr-wæqqas		

English	French	sg.	pl.	Comment
python	python I	t-a-žæ̀bdær-æt		perhaps *Gongylophis* (= *Eryx*) *muelleri* cf. Songhay *namey hasu*
python, rock	python II	t-ànæɣw-æt		*Python sebae*
roan antelope	hippotrague	a-šǽmol		perhaps the roan antelope *Hippotragus equinus*, now locally extinct like other antelopes. cf. Zenaga
viper	vipère	kætètuŋgu		*Cerastes cerastes* Songhay phrase "bring a wrap (= shroud)!"]
viper	vipère	s-æ̀ffæltæs		*Cerastes vipera* but also spitting cobra
viper, spotted	vipère	t-e-bæ̏ki-t-t		also horned *Cerastes cerastes*
vulture	vautour	e-žǽdær		*Gyps rueppellii*
vulture	vautour	t-æ̀-ɣalje		*Neophron percnopterus*,
vulture	vautour	e-jǽdær, e-žǽdær		*Gyps rueppellii*
warthog I	phacochère I	a-gæ̀ŋgæra		
warthog II	phacochère II	a-ẓæ̀ybæra		
zorilla	zorille	a-ræ̀ræŋŋa		*Ictonyx striatus*

Sudlow's Burkina Faso Tamachek dictionary (2009)

Table 8. Animal names in Tamachek of Burkina Faso

English	French	sg.	Comment
aardvark	oryctérope	adhəg	
addax	addax	aməllal	
antelope	antilope	əzăm	
antelope sp.	antilope sp.	tasărakənt	
baboon I	babouin I	kăya	
baboon II	babouin II	abiddăw	
bat	chauve-souris	afărtătta	
buffalo	buffle	esu n ăroǵǵ	
bustard	outarde	ăgayəs	
chameleon	caméléon	tawət	
cheetah	guépard	adal	
civet	civette	telḍət	
cobra, spitting	cobra cracheur	ăssăm (Z)	
cobra, spitting	cobra cracheur	emăǵăl (S)	
crane	grue	tenek	
crocodile	crocodile	ăɣata	
dama gazelle	gazelle dama	ener	cf. Zenaga
dorcas gazelle		ažənkəḍ	*Gazella dorcas*

English	**French**	**sg.**	**Comment**
elephant	éléphant	elu	
fennec fox	renard	ezăgăz	
francolin	francolin	ăfukăret	
gazelle sp.	gazelle sp	abugăra	
gazelle, red-fronted	gazelle à front rouge	edəmi	*Gazella rufifrons*
gerbil		eḍəwi	
giraffe	girafe	amdăɣ	
ground squirrel	écureuil de terre	ăkolăn	
guinea fowl	pintade	taylălt	
hare	lièvre	tekyăryălt	
hedgehog	hérisson	tekəneššit	
hippo	hippopotame	ăgamba	< Songhay
hyena, spotted	hyène, tachetée	təžori	*Crocuta crocuta*
hyena, striped	hyène, rayé	aridal	
jackal	chacal	ebəggi	*Canis adustus*
jackal	chacal	in-tănɣən	
leopard	panthère	wašil	
lion I	lion I	ăhărr	
lion II	lion II	ăxxu	
lion III	lion III	ebăɣăw	
monitor lizard	varan	ăɣata	
monkey	singe	abbidăw	patas monkey
monkey	singe	kăya	
mouse	souris	immi	
oryx	oryx	ezăm	
ostrich	autruche	anil	
ostrich	autruche	etăqq	Mali dialect
ostrich male	autruche, male	abal	
python	python	tanăɣwăt	
rat	rat	ăkundăr	
ratel	ratel	afăẓăẓa	
rhino	rhino	tin isək	
roan antelope	hippotrague	ažămol	
topi	topi	edəri	
viper	vipère	taššelt	
vulture	vautour	egădăr	cf. Zenaga
vulture	vautour	əẓiẓ	
vulture	vautour	tăɣalge	
warthog I	phacochère I	agăŋgăra	
warthog II	phacochère II	aẓăybăra, aẓebăra	
zorilla	zorille	fəkədərru	*Ictonyx striatus*
zorilla	zorille	ǵănǵăhila	*Ictonyx striatus*
zorilla	zorille	arărăŋŋa	*Ictonyx striatus* Mali

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

Proto-Nostratic Morphology

Allan R. Bomhard
Charleston, South Carolina, USA

1. Introduction

In 2000, Joseph Greenberg published the first volume, dealing with grammar, of his two-volume work *Indo-European and Its Closest Relatives*. In a paper published in 2004 in *Nostratic Centennial Conference: The Pécs Papers*, I presented some preliminary thoughts on the reconstruction of Proto-Nostratic morphology, building upon and expanding upon Greenberg's work. A revised and expanded version of that paper was incorporated into my 2008 book *Reconstructing Proto-Nostratic: Comparative Phonology, Morphology, and Vocabulary*. In the intervening years, I have continued my research and, as result, refined my views. In this paper, I would like to present a systematic reconstruction of Proto-Nostratic morphology taking into consideration my most recent findings (as of 2013). This paper incorporates, corrects, and expands upon my previous work.

According to Dolgopolsky (1994:2838):

> The parent language had, most probably, an analytic grammatical structure with a strict word order (sentence-final predicate; object preceding the verb; nonpronominal attribute preceding the head; a special position for unstressed pronouns) and with grammatical meanings expressed by word order and auxiliary words (e.g., postpositions: **nu* for genitive, **ma* for marked accusative, and others). In the descendant languages this analytic grammar evolved towards a synthetic one.

My own research tends to support Dolgopolsky's views. The evidence indicates that, in its earliest phases of development, the Nostratic proto-language had an analytic morphological structure, though, in its latest phases, a certain amount of evolution toward a synthetic structure must already have taken place, since a synthetic grammatical structure is reconstructed for Afroasiatic, which was the earliest branch to separate from the rest of the Nostratic speech community. That a good deal of this evolution took place within Proto-Afroasiatic proper is beyond doubt, inasmuch as a variety of analytic formations can be found in other branches of Nostratic, some of which can be traced back to the Nostratic parent language.

2. Proto-Nostratic as an Active Language

The assumptions we make about the morphological and syntactical structure of a given proto-language profoundly affect the reconstructions that we propose. For example, in what follows, I will be proposing that Proto-Nostratic was an active language. Now, active languages exhibit specific characteristics (see below) that set them apart from other morphological types. Therefore, it follows that the reconstructions I posit will conform with an active structure. However, I believe quite emphatically that reconstructions must never be driven by theory alone. Rather, they must be fully consistent with the supporting data. Moreover, not only must our reconstructions be consistent with the supporting data, they must be consistent from a typological perspective as well, and they must be able to account for later developments in the descendant languages in as straightforward a manner as possible, without recourse to ad hoc rules. When reconstructions are driven by theory alone, the results can be disastrous. Here, I will mention first the Moscow School (notably Illič-Svityč and Dolgopolsky) reconstruction of the Proto-Nostratic obstruent system as an example. On the basis of a few seemingly solid cognates in which glottalized stops in Proto-Afroasiatic and Proto-Kartvelian correspond to what are traditionally reconstructed as plain voiceless stops in Proto-Indo-European, Illič-Svityč assumes that voiceless stops in the Indo-European data he cites always means that glottalized stops are to be reconstructed in Proto-Nostratic, even when there were no corresponding glottalized stops in Afroasiatic and Kartvelian. He goes so far as to set up an ad hoc rule to account for counter-examples. Another example is Décsy's book (2002) on Proto-Afroasiatic. Here, Décsy makes certain ad hoc assumptions about what must have existed in language in general at a certain time depth and then applies those assumptions to his reconstruction of Proto-Afroasiatic. Though it is not known where or when human language first appeared, the fossil record indicates that anatomically modern humans have been around for approximately 190,000 years. That is more than enough time for language to develop. To assume that complicated linguistic structures could not have existed 12,000 years ago, a mere fraction of the length of time that our species has been on this planet, is not a view that I can support. It should be noted here that this criticism does not apply to Décsy's books on Uralic (1990), Indo-European (1991), and Turkic (1998) in the same series.

Several scholars have recently presented persuasive arguments in favor of reconstructing an early phase of Proto-Indo-European as an active language (cf. especially Karl Horst Schmidt 1980; Gamkrelidze—Ivanov 1995; and Lehmann 1995 and 2002). Proto-Afroasiatic is also assumed to have been an active language (cf. Diakonoff 1988:85), as is Elamite (cf. Khačikjan 1998:61—66). In active languages, subjects of both transitive and intransitive verbs, when they are agents semantically, are treated identically for grammatical purposes, while non-agent subjects and direct objects are treated differently (cf. Trask 1993:5—6; Dixon 1994:71—78). An "agent" may be defined as the entity responsible for a particular action or the entity perceived to be the cause of an action (cf. Trask 1993:11; Crystal 1992:11 and 2003:16).

Thus, there are two types of intransitive verbs in active languages (this will be explained in more detail below):

1. Those whose subjects have the same grammatical marking as the subjects of transitive verbs. These are Trask's "agent [subjects]". This type is referred to in this paper as "active constructions".
2. Those whose subjects have the same grammatical marking as direct objects of transitive verbs. These are Trask's "non-agent subjects". This type is referred to in this paper as "stative constructions".

To complicate matters, many verbs can be used either in a transitive sense or an intransitive sense. Semantic and morphosyntactic considerations play an important role here.

Trask's (1993:5—6) complete description/definition of active type languages is as follows:

> active language *n.* (also agentive language) A language in which subjects of both transitive and intransitive verbs which are semantically agents are treated identically for grammatical purposes, while non-agent subjects and direct objects are treated differently. Among languages exhibiting this pattern are Sumerian, Batsbi (NE Caucasian), Crow (Siouxan) and Eastern Pomo (Hokan). The following examples from Eastern Pomo show the use of the two subject pronouns *há:* 'I' (agent) and *wí* 'I' (non-agent): *Há: mí:pal šá:ka* 'I killed him'; *Há: wádu:kìya* 'I'm going'; *Wí ʔéčkiya* 'I sneezed'. The correlation is rarely perfect; usually there are a few verbs or predicates which appear to be exceptional. In some active languages lexical verbs are rigidly divided into those taking agent subjects and those taking non-agent subjects; in others some lexical verbs can take either to denote, for example, differing degrees of control over the action. See Merlan (1985) for discussion. Cf. ergative language, accusative language, and see also split intransitive, fluid-intransitive. Sapir (1917).

Nichols (1992:9—10) lists the sets of typical features of active type languages established by Klimov (1977) as follows:

> Lexical properties:
>
> 1. Binary division of nouns into active vs. inactive (often termed *animate* and *inanimate* or the like in the literature).
> 2. Binary division of verbs into active and inactive.
> 3. Classificatory verbs or the like (classification based on shape, animacy, etc.).
> 4. Active verbs require active nouns as subject.
> 5. Singular-plural lexical suppletion in verbs.
> 6. The category of number absent or weakly developed.
> 7. No copula.

8. "Adjectives" are actually intransitive verbs.
9. Inclusive/exclusive pronoun distinction in first person.
10. No infinitive, no verbal nouns.
11. Etymological identity of many body-part and plant-part terms (e.g., "ear" = "leaf").
12. Doublet verbs, suppletive for animacy of actant.

Syntactic properties:

13. The clause is structurally dominated by the verb.
14. "Affective" (inverse) sentence construction with verbs of perception, etc.
15. Syntactic categories of nearer or farther object rather than direct or indirect object.
16. No *verba habiendi.*
17. Word order usually SOV.
18. Direct object incorporation into verb.

Morphological properties:

19. The verb is much more richly inflected than the noun.
20. Two series of personal affixes on the verb: active and inactive.
21. Verbs have aspect or Aktionsarten rather than tense.
22. The noun has possessive affixes.
23. Alienable-inalienable possession distinction.
24. Inalienable possessive affixes and inactive verbal affixes are similar or identical.
25. Third person often has zero affix.
26. No voice opposition (since there is no transitivity opposition). Instead, there can be an opposition of what is called *version* in Kartvelian studies (roughly active vs. middle in the terminology of Benveniste 1966, or an opposition of normal valence vs. valence augmented by a second or indirect object, or an opposition of speech-act participant vs. non-participant in indirect-object marking on the verb).
27. Active verbs have more morphological variation or make more morphological distinctions than inactive verbs.
28. The morphological category of number is absent or weakly developed.
29. There are no noun cases for core grammatical relations (no nominative, accusative, genitive, dative). Sometimes there is an active/inactive case opposition.
30. Postpositions are often lacking or underdeveloped in these languages. Some of them have adpositions inflected like nouns.

Nichols (1992:8) notes that Klimov's definition of active type languages is close to, though not identical with, her definition of dominant stative-active alignment (see also Nichols 1992:8—9):

> According to Klimov, the basic determinant of linguistic type is what I call the *conceptual cast* of a language's predictions and its categorization of basic nominal and verbal notions; whether they are based on subject-object relations, agent-patient relations, an active/inactive distinction, referential properties, or others. The salient indicator of the conceptual cast is the stative-active, ergative, or accusative alignment of the clause, and this in turn determines the occurrence of a number of other categories. The

> whole set of properties — conceptual cast, alignment type, and attendant categories — constitutes the *type* of the language. (Klimov 1977 divides the relevant grammatical features into those that are more or less direct implicanda of type and those that are frequently observed secondary properties.) There are four basic types: the ACCUSATIVE TYPE, which grammaticalizes subject-object relations, the ERGATIVE TYPE, which grammaticalizes agent-factitive relations (for *factitive* — a semantic role essentially coinciding with the formal category of S/O of Dixon 1979 — see Kibrik 1979); the ACTIVE TYPE, which grammaticalizes an active/inactive or animate/inanimate principle; and (singled out only in the 1983 book) the CLASS TYPE, based on referential properties of nominals and having well-developed gender or class inflection. The first three types are named for their typical clause alignments, but in Klimov's view clause alignment is merely one of several symptoms (albeit a salient one) of the conceptual cast and hence type. Thus the active type is almost identical in extension but different in intension from the set of languages exhibiting stative-active alignment. Since the active type is focal in Klimov's sense, I will use his term *active* in his sense while using *stative-active* in what I take to be the current standard sense. Klimov carefully distinguishes type from features, faulting most contemporary typology for failing to make this distinction and pointing out that much of what is called typology is actually the cross-linguistic study of features rather than types. A type, in Klimov's view, is a set of independent but correlated features from different levels of grammar accompanied by a theory explaining the correlation.
>
> What is of particular interest to cross-linguistic comparison is the sets of typical features Klimov establishes for each type. For instance, he shows that the active type is associated with underdevelopment of number inflection, an inclusive/exclusive opposition in pronouns, an opposition of alienable to inalienable possession, classificatory verbs, grammaticalized animacy in nouns, and other features. The active and class types display the largest number of distinctive, interesting, and testable properties, and it is these traits that will be surveyed here.

Nichols (1992:65—66) describes various types of clause alignment as follows — note, in particular, her description of stative-active alignment (e):

> 2.0.4. *Clause alignment*. This term (taken from relational grammar) will be used here as generic for accusative, ergative, stative-active, etc. Only morphological alignment is surveyed in this study. The following categories are used, based on the morphological distinction or nondistinction of A, O, S (as those abbreviations are used by Dixon 1979 to refer to subject of transitive, direct object, and subject of intransitive respectively). The first five are standard and the last, hierarchical, is a well-described pattern with no standard label (Mallinson and Blake 1981 use the term *relative-hierarchical*).
>
> (a) Neutral: A = O = S, i.e., no inflectional oppositions.
> (b) Accusative: S = A; O distinct.
> (c) Ergative: S = O; A distinct. When a language has a major tense- or person-based ergative/accusative split and both patterns are salient, I count the language as primarily ergative, on the grounds that (following Silverstein 1976) most ergative systems are split and hence the split is part of the definition of "ergative".
> (d) Three-way: A, O, and S all distinct.
> (e) Stative-Active: S_1 = A, S_2 = O, the language has two different kinds of intransitive verbs, one taking ordinary subject marking (or the same subject marking as used with transitive verbs) and the other taking a subject whose marking is the same as that of the direct object of a transitive. The choice of S_1 or S_2 is usually determined by the verb: "stative" verbs take S_2, "active" verbs S_1. (For this definition see Merlan 1985.)

> If S_1 = A is the clear majority type in stative-active languages, the language can be described as having an accusative bias or slant: most intransitive subjects are formally identical to transitive subjects, so for the most part S = A. If S_2 = O is the clear majority type, the language has an ergative bias. I will speak of such languages as being stative-active on an accusative BASE or stative-active on an ergative base.
>
> (f) Hierarchical: Access to inflectional slots for subject and/or object is based on person, number, and/or animacy rather than (or no less than) on syntactic relations. The clearest example of the hierarchical type in my sample is Cree. The verb agrees in person and number with subject and object, but the person-number affixes do not distinguish subject and object; that is done only by what is known as direct vs. inverse marking in the verb. There is a hierarchical ranking of person categories: second person > first person > third person. The verb takes direct marking when subject outranks object in this hierarchy, and inverse marking otherwise. In addition, verbs inflect differently depending on whether their S and O arguments are animate or not, a pattern which could be viewed either as another instance of hierarchical agreement or as different conjugation classes (rather than hierarchical access to agreement slots).

Next, Nichols (1992:100—105) describes head/dependent marking and alignment with regard to the various types of clause alignment mentioned above as follows (the tables given in the original are omitted here):

> The frequencies of the dominant alignment types exhibited by the various head/dependent types are shown in table 18. The accusative alignment has almost the same distribution as the total of all three alignment types; in other words, its distribution is not affected by head/dependent marking and we can conclude that it is equally compatible with all head/dependent types. The ergative alignment favors dependent-marking morphology: of the 28 ergative languages in the sample, 16 are dependent-marking and only four are strongly head-marking (Abkhaz, Wishram, and Tzutujil, all with 0.0 proportions; Yimas with 0.25). The ergative type is well installed and stable in these languages, however: the first three (Abkhaz, Wishram, Tzutujil) belong to well-described families (Northwest Caucasian, Chinookan, Mayan) that are consistently ergative.
>
> The stative-active and hierarchical types strongly prefer head-marking morphology, consistent with the fact that the verb is the favored part of speech for showing stative-active marking. It is of course possible for a dependent-marking language to have stative-active dominant alignment. The dependent-marking stative-active languages in my sample, plus one (Batsbi; see Holisky 1987) not in my sample, are listed below, with their head/dependent ratios, alignment of noun and verb, and whether the structural semantics of the oppositions is of the split-S or fluid-S type in the terms of Dixon 1979.
>
> The fluid-S type is rare overall among stative-active languages (Merlan 1985), and these examples show that the fluid-S type has a strong affinity for case-marking languages. Head-marking stative-active languages are split-S with only one exception. Acehnese uses head marking to implement a fluid-S type (Durie 1985:185ff.). We can conclude that the unmarked kind of stative-active language is head-marking and split-S.
>
> The correlation of head/dependent marking and alignment emerges more clearly if we plot the head-marking points in the clause against the alignment of the verb, as shown in table 19. The high frequency of neutral alignment in languages with no head marking in the clause is to be expected by definition: a language having no clause head marking has no marking on the verb, and no marking is neutral alignment. What requires comment is the non-neutral examples with zero clause head-marking. These include two languages that use detached marking, which I somewhat arbitrarily counted as marking of

alignment on the verb. These two languages are Haida (stative-active) and Luiseño (accusative). Otherwise, once again the distribution of the accusative alignment is much like that of the total, and the stative-active and hierarchical alignments are concentrated in the head-marking end of the scale (higher numbers of H points in S). The ergative alignment is fairly evenly distributed throughout the scale except that it does not occur in languages with zero head marking in the clause (since ergativity cannot be marked on the verb if the verb has no marking).

It is apparently possible to combine any of the three major alignment types with any head/dependent type, though there are preferred and dispreferred combinations and there are gaps (which I interpret as accidental) in the distribution of the low-frequency types. The accusative alignment is equally compatible with all types, as is consistent with its generally preferred and unmarked status. The less frequent types have interesting asymmetries and limitations. The ergative alignment favors dependent marking. This is consistent with the fact that ergative, of all alignment types is prone to be marked on the noun (see §2.3.1), and this in turn may have to do with the fact that ergative alignment grammaticalizes nominal semantic roles. Stative-active and hierarchical alignments prefer head marking, and this is consistent with what they grammaticalize: the stative-active type grammaticalizes lexical categories of verbs, and the hierarchical type grammaticalizes relative ranking (for referential properties: animacy, person, etc.) rather than absolute functional status of clause arguments. The dependent-marked stative-active type is generally fluid-S, which is to say that it codes nominal semantic roles and not verb categorization. In general, the alignments that favor marking on nominals (ergative; fluid-S stative-active) are associated with grammatical-ization of nominal semantic functions; those that favor marking on verbs are associated with the grammaticalization of verbal semantics and/or the semantics of the whole clause. Thus we have a functional explanation, albeit a rather abstract one. But on a more general level, the distributional constraints on alignment types suggest that there is some kind of consistency between the morphological form of coding (head-marked or dependent-marked) and the semantics coded; fluid categories and NP relational semantics favor dependent marking, while split categories and verbal notions favor head marking. If the function of the part of speech bearing the marking influences the semantics coded, it is also true that the form of the coding, specifically its location, restricts its possible semantics.

The correlation of stative-active type with head marking and ergative with dependent marking is difficult to demonstrate areally, partly because nonaccusative alignments are not common enough to form clear patterns in any but the largest areas and partly because ergative and stative-active alignments are roughly in complementary distribution across the areas. Table 20 shows that wherever the ergative alignment is at all frequent it is associated with dependent marking, and wherever the stative-active alignment is frequent it is associated with head marking. Even when neither is frequent, as in the smaller areas, there is still conformity in that in most cases the few stative-active entries are no more dependent-marking, and often more nearly head-marking, than the few ergative entries. The only counterexample is the Caucasus. The correlation emerges as significant by Dryer's test (reliably so if only the six continent-sized areas are considered; less reliably, but numerically more strongly, if all areas are counted).

As mentioned in §2.0.4, stative-active languages can be described as having an ergative or accusative base, depending on whether the object-inflecting ("stative") or subject-inflecting ("active") set of intransitives is an open set. A base alignment can also be determined by considering the nominal and pronominal inflection, and sometimes also the inflection of transitive verbs. Information on closed and open classes of intransitives is not always available, but where available it indicates that most stative-active languages have an accusative base. Inflectional paradigms yield the same conclusion: ergative base alignment is rare outside of the Old World (where it is found in Georgian and Elamite). Languages with hierarchical dominant alignment have an accusative or neutral base without exception.

Regarding Georgian, Nichols (1992:314, note 3) remarks:

> Georgian is classified as stative-active because of its split transitivity. Hewitt 1987 gives detailed arguments against it on the grounds that the semantics of agent and patient does not determine case choice in intransitive subjects, but my definition of stative-active is not based on nominal semantic roles. Klimov 1977, 1983a classifies Georgian as belonging to the active type, although his classification is not based entirely on alignment: see the summary of his typology in §1.1.1 above.

Finally, Nichols (1992:116—117) discusses alienable and inalienable possession and its relationship to stative-active structure:

> Klimov 1977 finds that an opposition of alienable/inalienable possession is associated with the stative-active type. Nichols 1988, a survey limited to North America and Northern Eurasia, argues that the association is rather with head/dependent marking: inalienable possession almost always involves head marking, and head marking in NP's almost always entails an alienable/inalienable opposition. Chappell and McGregor 1989 give a more comprehensive structural analysis along comparable lines, placing alienable and inalienable possession in a hierarchy which continues on to lexical compounds and classificatory nouns. (Welmers 1971:132ff. finds evidence for a further connection — in this case historical rather than typological — of bound vs. free possession with nominal classes.) The present survey has supported most of the findings of Nichols 1988 and Chappell and McGregor 1989. Only possessive constructions taking the form of NP's are surveyed here.
>
> In the literature, the opposition of inalienable to alienable possession is generally presented as a semantic one, but Chappell and McGregor 1989 and Nichols 1988 show that it is best approached as a structural opposition rather than a semantic one. Languages with an opposition of inalienable to alienable possession have split systems of possession marking, and alienable and inalienable are not cross-linguistic semantic constants but simply the extremes of the nominal hierarchy defined by the splits. The term *inalienable*, then, refers not to a semantic constant having to do with the nature of possession, but to whatever set of nouns happens to take inalienable possession marking in a given language. In terms of its grammatical form, inalienable possession always involves a tighter structural bond or closer connection between possessed and possessor, and the tightness of the bond can be described in terms of head and dependent marking. One of the most common patterns is that where possession is head-marked and there is no formal difference between alienable and inalienable possession, other than that there is an inalienable set of nouns that cannot occur with possessive affixation while alienables can be used alone. In some languages there is a formal difference between alienable and inalienable possessive affixes: both are head-marking, and those for inalienables are shorter, simpler, or more archaic than those for alienables...
>
> There are several recurrent types of splits in the marking of possession, and all of them lend themselves to a single generalization: the inalienables take marking which is more nearly head-marking or less dependent-marking than the marking of alienables. Commonly, inalienable possession is head-marked while alienable is dependent marked...
>
> The generalizations to be made about inalienable possession thus resemble, in the abstract, those made in §3.2 about the stative-active alignment: both are associated with head marking, and both involve split rather than fluid systems. Stative-active alignment is typically but not necessarily split (occasionally as fluid, as in Batsbi, Acehnese, Eastern Pomo, and Tonkawa) and typically but not necessarily associated with head marking (occasionally with dependent marking, as in Batsbi, Eastern Pomo, and

> Tonkawa). Inalienable possession appears to be necessarily split (never fluid) and necessarily associated with head/dependent marking. The correlation with head/dependent marking is shown in the fact that no language in my sample (and no language that I know of) uses only dependent marking to implement an alienable/inalienable distinction. (A language that did so would have two genitive cases, one for alienables and one for inalienables.) Inalienable possession is split rather than fluid in that the choice of marking is determined by the possessed noun rather than by the speaker's decision about semantics. No language has what one would want to call fluid possessive marking, which would require the speaker to decide, for each possessed noun, whether (say) the possessor could part with the possessed item, whereupon the speaker would choose the formal marking accordingly…

Additional information on the salient morphological characteristics of active type languages is presented at the beginning of Chapter 19 of my 2008 book, especially as it pertains to positing an active-type structure for an early period of development in Proto-Indo-European. See also Dixon 1994:71—78.

3. Proto-Nostratic Phonological System

Proto-Nostratic had a rich system of stops and affricates. Each stop and affricate series was characterized by the three-way contrast: (1) voiceless (aspirated), (2) voiced, and (3) glottalized. The aspiration of series (1) was phonemically non-distinctive.

The Proto-Nostratic phonological system may tentatively be reconstructed as follows (cf. Bomhard 2008.I:213—214 and 2011:8—9):

Stops and Affricates:

p^{h}	t^{h}	c^{h}	č^{h}	t^{yh}	t͜ɬ^{h}	k^{h}	k^{wh}	q^{h}	q^{wh}		
b	d	ʒ	ǯ	d^{y}	d͜ɮ (?)	g	g^{w}	ɢ	ɢ^{w}		
p’	t’	c’	č’	t’^{y}	t͜ɬ’	k’	k’^{w}	q’	q’^{w}	ʔ	ʔ^{w}

Fricatives:

		s	š	s^{y}		x	x^{w}			h	ħ
		z	ž (?)	z^{y} (?)		γ					ʕ

Glides:

w				y							

Nasals and Liquids:

m	n			n^{y}		ŋ					

l	l^y
r	r^y

(It may be noted that the above reconstruction is extremely close to what Ehret [1980:37] posits for Proto-Southern Cushitic, but without the retroflex and pre-nasalized sounds.)

Vowels:	i (~ e)				u (~ o)
		e		o	
			(ə ~) a		

Also the sequences:	iy (~ ey)	uy (~ oy)	ey	oy	(əy ~) ay
	iw (~ ew)	uw (~ ow)	ew	ow	(əw ~) aw

As can be seen, the phonological system reconstructed above for Proto-Nostratic resembles that of Proto-Afroasiatic more closely than it does the phonological systems of any of the other branches. This is as it should be, inasmuch as Afroasiatic was the oldest branch, the first to become separated from the rest of the Nostratic speech community. Likewise, Proto-Afroasiatic, together with Proto-Dravidian, are of paramount importance for the reconstruction of Proto-Nostratic morphology.

4. Remarks on the Vowels

The following vowels may be reconstructed for Proto-Nostratic: **a*, **e*, **i*, **o*, and **u*. At least some of these vowels must have been subject to considerable subphonemic variation in the Nostratic parent language. The high front and back vowels **i* and **u*, in particular, may be assumed to have had lowered variants (indicated in the Proto-Nostratic reconstructions as **e* and **o* respectively), while the central low vowel **a* may be assumed to have had higher variants (indicated in the Proto-Nostratic reconstructions as **ə*). To complicate matters, **e* and **o* must also have existed as independent vocalic elements. It was the reanalysis, phonemicization, and exploitation of this subphonemic variation that gave rise, at least in part, to the ablaut and vowel harmony patterning found in the majority of the Nostratic daughter languages. It may be noted here that, according to Greenberg (1990), traces of an earlier system of vowel harmony can be discerned in Proto-Indo-European.

It is unclear whether phonemic long vowels existed in Proto-Nostratic as well, though the evidence seems to indicate that they did not, except, probably, in nursery words.

Finally, it may be noted that, while any vowel (**a*, **e*, **i*, **o*, **u*) could appear in initial syllables, only **a*, **i*, **u* could appear in non-initial syllables. This is identical to the patterning found in Dravidian.

The Proto-Nostratic vowels were, for the most part, preserved in initial syllables in Uralic, Dravidian, and Altaic. They appear to have been originally preserved in Proto-Afroasiatic as well. Within Afroasiatic, Cushitic and Omotic are particularly conservative in their vocalism, while the vowel systems found in Semitic, Egyptian, and Berber exhibit a wholesale reduction of the inherited system.

The system of vowel gradation found in Semitic, Egyptian, and Berber initially arose through morphological processes that will be discussed later in this paper. It appeared quite early in verbal stems and derivative nominal stems, though primary root nouns continued to maintain stable vocalism right up to the emergence of the individual daughter languages. Once established, the system of vowel gradation was greatly expanded, especially in Semitic.

The inherited vowel system underwent a thorough restructuring in both Proto-Indo-European and Proto-Kartvelian as a result of a complicated series of changes initiated by the phonemicization of a strong stress accent in the early prehistory of these branches. These developments diminish the importance of Kartvelian and Indo-European for ascertaining the Proto-Nostratic vowel system.

5. Ablaut in Proto-Nostratic

An analysis of the Afroasiatic (and, to a lesser extent, Dravidian) data seems to indicate that there was an alternation between the vowels **a*, **i*, and **u* in Proto-Nostratic roots and that that alternation had some sort of morphological or semantic significance. This is most clear in the Proto-Afroasiatic reconstructions proposed by Orël—Stolbova (1995), where different root vowels are sometimes posited by them for two (or more) stems, all of which are clearly variants of the same root. Each stem is listed by them as a separate entry, though the stem is usually cross-referenced to the related entry or entries. It should be mentioned that the same patterning is evident in Ehret's (1995) reconstructions. At the present state or research, however, it is simply not possible to ascertain the details of that patterning and what that patterning may have signified.

6. Root Structure Patterning in Proto-Nostratic

Comparison of the various Nostratic daughter languages makes it possible to determine the rules governing the structural patterning of roots and stems in Proto-Nostratic. Most likely, the patterning was as follows:

1. There were no initial vowels in Proto-Nostratic. Therefore, every root began with a consonant.

2. There were no initial consonant clusters either. Consequently, every root began with one and only one consonant. Medial clusters were permitted, however.
3. Two basic root types existed: (A) **CV* and (B) **CVC*, where *C* = any non-syllabic, and *V* = any vowel. Permissible root forms coincided exactly with these two syllable types.
4. A stem could either be identical with a root or it could consist of a root plus a single derivational morpheme added as a suffix to the root: **CVC*+*C*-. Any consonant could serve as a suffix. Note: In nominal stems, this derivational suffix was added directly to the root: **CVC*+*C*-. In verbal stems, it was added to the root plus formative vowel: **CVC*+V+*C*-.
5. A stem could thus assume any one of the following shapes: (A) **CV*-, (B) **CVC*-, (C) **CVC*+*C*-, or (D) (reduplicated) **CVC*-*CVC*-. As in Proto-Altaic, the undifferentiated stems were real forms in themselves and could be used without additional suffixes or grammatical endings. However, when so used, a vowel had to be added to the stem: (A) **CV*- > **CV* (no change), (B) **CVC*- > **CVC*+V, (C) **CVC*+*C*- > **CVC*+*C*+V, or (D) (reduplicated) **CVC*-*CVC*- > **CVC*-*CVC*+V. Following Afroasiatic terminology, this vowel may be called a "terminal vowel" (TV). Not only did terminal vowels exist in Proto-Afroasiatic (cf. Ehret 1995:15; Bender 2000:214—215 and 2007:737—739), they are also found in Dravidian, where they are called "enunciative vowels" (cf. Steever 1998:15; Krishnamurti 2003:90—91; Zvelebil 1990:8—9), and in Elamite (cf. Khačikjan 1998:11; Grillot-Susini 1987:12), where they are called "thematic vowels". In Proto-Dravidian, the enunciative vowel was only required in stems ending in obstruents, which could not occur in final position.

Concerning enunciative vowels in Dravidian, Zvelebil (1990:8—9) notes:

> No obstruents can occur finally. When they do, they are followed by a "non-morphemic" automatic (so-called epenthetic, or 'enunciative' or 'euphonic', i.e. predictable morphophonemic) vowel *-ə which is regularly dropped according to morphophonemic rules…

While Krishnamurti (2003:90—91) writes:

> If the stem ends in a stop, it is followed by a non-morphemic or enunciative vowel /u/. Roots of (C)VC- and (C)VCC- contrast when followed by formatives or derivative suffixes beginning with vowels. It is not clear if the difference between root-final C and CC is determined by the nature of the derivative suffix that follows. When roots in final obstruents are free forms, the final consonant is geminated followed by a non-morphemic (enunciative) *u*. When roots of the type (C)V̄C- or (C)VCC- are followed by a formative vowel, V_2 = /i u a/, they merge with (C)VC-.

Ehret (1995:15) makes the following observations about the terminal vowels in Proto-Afroasiatic:

> The Omotic, Cushitic, and Chadic evidence conjoin in requiring the existence in PAA of an additional element in word formation, a terminal vowel (TV) in nouns and modifiers, the original function and meaning of which remain obscure. TVs have been subjected to comparative-historical investigation in only two groups of Afroasiatic languages. In Omotic they have no reconstructible function beyond their necessary attachment to singular noun stems in semantically predictable fashion. With the exception of Kafa, in which two TVs, *-o* and *-e*, have been grammaticalized respectively as masculine and feminine markers, they carry no grammatical or recognizable semantic load (Hayward 1987). In proto-Southern Cushitic, pairs of TVs formed a variety of singular-plural markers. Particular paired sets tended to go with either masculine or feminine nouns, but an individual TV on a singular noun generally gave no indication of the grammatical gender of that noun (Ehret 1980:49—50).
>
> From these indicators it seems reasonable to conclude that TVs are fossils of a nominal morphology productive in pre-proto-Afroasiatic and predating the rise of grammatical gender in the family. Having lost their original grammatical function, they have been reanalyzed as markers of the singular or sometimes, as in the case of Southern Cushitic, of the plural in nominals. In the BoreAfroasiatic subgroup (Semitic, Egyptian, and Berber: see Chapter 6 for this classification), the TVs have generally been dropped entirely, leaving most nouns and adjectives as consonant-final words.
>
> The existence of TVs at early stages of Afroasiatic evolution obviates the need to reconstruct any syllabic consonants for PAA. The usual word structure of nouns and adjectives would have been $*C_1(VC_2)(C_s)V_{tv}$, in which the only possible structures are CVC and CV and never just C. The presence of syllabic C in BoreAfroasiatic languages can be understood as the natural outcome of vowel loss, whether word-internal or word-final, within that particular subgroup (as is also separately the case in a few modern Omotic languages, notably Bench and Maji, where the same kind of sound change has independently been at work).

While Bender (2000:214—215) makes the following comments about Omotic:

> Hayward (1987, 1980a, 1980b) reported in some detail on the matter of "terminal vowels" (TVs) found in sg. nouns in Ometo languages and Ari. Hayward states that the TVs in Ari are often independent of the root (1990b:440) and that in Zaysé, they are appendages, not part of the root, but being unpredictable, must be included in lexical entries (1990a:242). In some cases, final vowels distinguish gender. This is much more the case with pronominals, but I restrict the term "TVs" to the nominal category in non-derived and non-inflected form (except insofar as TV may mark gender)...
>
> In the 1990c article, variation of vowels beyond the "cardinal" *i*, *e*, *a*, *o*, *u* did not seem to be significant in TVs. TVs are prominent in all branches except Gimira, where CVC is the norm, with tone carrying a high functional load. It would be tidy if TVs were reconstructable: they would thus be predictable across languages if not within languages according to lexical items. But first of all, there is no unanimity among the sources: different investigations record different TVs and even one source may have alternative forms.

As noted above, terminal vowels are only used with nouns and modifiers in Afroasiatic, while in Dravidian, the single reconstructible terminal vowel, **-u*, is used after any free-form stem ending in an obstruent. For Proto-Nostratic, the patterning may be assumed to have been as follows: If an undifferentiated stem (nominal or verbal) of the type ending in a consonant was used as a free-form, a terminal vowel had to be added. In

Proto-Nostratic, the terminal vowels were: **a*, **i*, and **u*. The origin of terminal vowels will be investigated below.

The original root structure patterning was maintained longer in Afroasiatic, Dravidian, and Altaic than in the other branches, while the patterning found in Proto-Indo-European and Proto-Kartvelian has been modified by developments specific to each of these branches. The root structure constraints found in Proto-Indo-European were an innovation. In Proto-Uralic, the rule requiring that all words end in a vowel was an innovation and arose from the incorporation of the so-called "terminal vowel" into the stem. It should be noted that reduplication was a widespread phenomenon in Proto-Nostratic. It was one of the means used to indicate plurality in nouns, while, in verbs, it may have been used in frequentive and habitual formations.

On the basis of the evidence of Proto-Indo-European, Proto-Kartvelian, Proto-Afroasiatic, Proto-Dravidian, and Proto-Altaic, it may be assumed that there were three fundamental stem types: (A) verbal stems, (B) nominal and adjectival stems, and (C) pronominal and indeclinable stems. Some stems were exclusively nominal. In the majority of cases, however, both verbal stems and nominal stems could be built from the same root. In Proto-Nostratic, only pronominal and indeclinable stems could end in a vowel. Verbal and nominal stems, on the other hand, had to end in a consonant, though, as noted above, when the undifferentiated stems were used as real words in themselves, a "terminal vowel" had to be added to the stem. As we shall see below, the "terminal vowels" were morphologically significant. Adjectives did not exist as an independent grammatical category in Proto-Nostratic.

As in Proto-Kartvelian, it appears that Proto-Afroasiatic underwent several syntactic shifts in its prehistoric development. Surely, the VSO pattern found in Semitic, Egyptian, and Berber is an innovation. While it is not possible to trace the exact developments, it seems likely that the original pattern was SOV, which is what is found in the majority of Cushitic languages. Ehret (1995:52) arrives at the same conclusion for Proto-Afroasiatic. He notes that nominalizing morphology in Proto-Afroasiatic was predominantly suffixal. One little aside: The more I look at the matter, the more I am convinced that, within Afroasiatic, Semitic is the most aberrant branch. In view of this, notions of what Proto-Afroasiatic might have been like, based primarily upon the Semitic model, are likely to be false.

7. The Prehistory of Root Structure Patterning and the Development of Terminal Vowels

During the earliest period of Proto-Nostratic, *roots* could only have the forms: (a) **CV*- and (b) **CVC*-. Type (a) was restricted to pronominal stems and indeclinables, while type (b) characterized nominal and verbal stems. A single *derivational suffix* could be placed after root type (b): **CVC+C* (derivational suffix). Grammatical relationships were indicated by placing *particles* either after the undifferentiated stem or after the stem plus a derivational suffix: (a) **CVC+CV* (particle [P]) or (b) **CVC+C* (derivational suffix [DS]) + *CV* (particle [P]). In nominal stems, a morphologically significant *terminal vowel* (TV) had to be added directly after the root, while in verbal stems, a *formative vowel* (FV) had to be added between the root and any following element, be it derivational suffix or particle; thus, we get the following patterns:

(a) (noun stem) $^{*}\mathrm{CVC}(+\mathrm{C}_{\mathrm{DS}})+\mathrm{V}_{\mathrm{TV}}$	(plus particle): $^{*}\mathrm{CVC}(+\mathrm{C}_{\mathrm{DS}})+\mathrm{V}_{\mathrm{TV}}+\mathrm{CV}_{\mathrm{P}}$
(b) (verb stem) $^{*}\mathrm{CVC}+\mathrm{V}_{\mathrm{FV}}(+\mathrm{C}_{\mathrm{DS}})$	(plus particle): $^{*}\mathrm{CVC}+\mathrm{V}_{\mathrm{FV}}(+\mathrm{C}_{\mathrm{DS}})+\mathrm{CV}_{\mathrm{P}}$

The derivational suffixes were derivational rather than grammatical in that they affected the meaning of a word rather than its relation to other words in a sentence.

This is essentially the stage represented in Proto-Dravidian, though Proto-Dravidian has added long vowels to the equation as well as stems beginning with a vowel (no doubt arising from the loss of initial laryngeals) (cf. Krishnamurti 2003:179—184 and 277—279). Next, the formative vowel was reinterpreted as part of the derivational suffix in verbal stems: **CVC+VC+CV*. This is the stage represented by Proto-Afroasiatic (cf. Diakonoff 1988:85—110; Ehret 1995:15 and 27—34) and is the basis for the root structure patterning found in Proto-Kartvelian and Proto-Indo-European as well. From an Afroasiatic perspective, there is no such thing as "formative vowels" — they are only preserved in Dravidian and Elamite, though, in Elamite, their status is disputed (cf. Reiner 1969:78).

In Proto-Dravidian, the original meaning of the formative vowel was completely lost. According to Krishnamurti (2003:97), "[i]t apparently had an epenthetic role of splitting clusters without affecting the syllable weight …" Note the following examples given by Krishnamurti (2003:181):

1. **tir-a-y-* (**-p-/*-mp-*, **-nt-*) 'to roll (intr.)'; **tir-a-y-* (**-pp-/*-mpp-*, **-ntt-*) 'to roll up (tr.)', (n.) **tir-a-y* 'wave, screen, curtain'; **tir-a-nku* 'to be curled up (intr.)', **tir-a-nkku* 'to shrivel (tr.)';
2. **tir-a-ḷ-* (**-p-*, **-ṇṭ-*) 'to become round (intr.)', **tir-a-ḷ-* (**-pp-*, **-ṇṭṭ-*) 'to make round (tr.)';
3. **tir-i-* (**-p-*, **-nt-*) 'to turn (intr.)', **tir-i-* (**-pp-*, **-ntt-*) 'to turn (tr.)'; **tir-u-ku* 'to twist (intr.)', **tir-u-kku* 'to twist (tr.)'; **tir-u-mpu* 'to twist, to turn (intr.)', **tir-u-mppu* 'to twist, to turn (tr.)';
4. **tir-u-ntu* 'to be corrected, to be repaired (intr.)', **tir-u-nttu* 'to correct, to rectify (tr.)'.

As stated by Krishnamurti (2003:181), "[t]he Proto-Dravidian root is obviously **tir-*, meaning 'turn, roll, twist, change shape' → 'correct', etc. The formatives occur in two layers. The first layer is V = *i*, *a*, *u*; and the second layer, either a sonorant (L) as in *y*, *ḷ*; or a simple or geminated stop ± homorganic nasal: P as in **ku*; PP as in **kku*; NP as in **nku*, **ntu*, **mpu*; NPP as in **nkku*, **nttu*, **mppu*."

In Elamite, verbal stems consisted either of a root ending in a vowel or of a root extended by a thematic vowel if the root ended in a consonant: *kuk-i* 'to protect' (< *kuk-*) (cf. Khačikjan 1998:13). Khačikjan (1998:11) also notes:

> Elamite was an agglutinative suffixal language. The suffixes joined either the root or the stem.
>
> The root morpheme consisted mostly of two consonants and one or two vowels: *nap* 'deity', *ruh* 'man', *zana* 'lady', *kap* 'treasure', *kik* 'sky', etc.
>
> The stem consisted of a root ending in a consonant, with thematic vowels *-i*, *-u*, *-a*, cf. *per-i-*, *mur-u-*, *tahh-a-* (< *tah-*). The thematic vowels *-u* and *-a* were only attested with verbal stems, whereas *-i* with nominal and nomino-verbal ones: *tir-i-* 'to speak', *kukk-i* 'vault, roof', *peti-* 'enemy; to revolt'.

Reiner (1969:78) notes, likewise, that the Elamite verb base always ended in a vowel: CVCV, CVCCV, and, though more rarely than the first two types, CV. Reiner argues against treating the thematic vowel ("stem-vowel") as a separate morpheme. Khačikjan, however, follows Paper in considering the thematic vowel to be a separate morpheme. Grillot-Susini (1987:32) simply states: "The structure of the verb is analogous to that of the noun. It consists of a base (simple root or enlarged by *-i*/*u*/*a*) to which the inflections of the verbal conjugation, the participial formants, and/or the nominal person suffixes are attached."

Now, it is curious that the formative vowel can take different shapes in Proto-Dravidian: **a*, **i*, or **u*. This seems to indicate that the different formative vowels must have had some sort of morphological significance at an earlier point in time, though this distinction was lost in Proto-Dravidian proper. Not only must the formative vowels have had morphological significance, the terminal vowels must also have had morphological significance.

The formative vowels found in verbal stems may have been aspect markers, as Zaborski has tried to show for Omotic. Here, according to Zaborski, the patterning was as follows: *a* marks present (imperfective), *i* ~ *e* mark past (perfective), and *u* ~ *o* mark subordinate. Though originally supportive of Zaborski's views, Bender later became skeptical, pointing out that he finds the consonantal markers to be more significant. Indeed, for Omotic or even Afroasiatic, this is what we would expect. But Zaborski's views are not so easily dismissed. What he may have uncovered is a more archaic pattern, as Bender himself admits. In Finno-Ugrian, the ending **-i-* shows up as a past tense marker. Likewise in Dravidian, where the suffix **-i-* is one of several used to mark past tense. These may ultimately be derived from a perfective marker **-i-*.

As noted above, when the unextended root (**CVC-*) served as the verbal stem, the formative vowel (aspect marker) was added directly to the root: $*CVC+V_{FV}$.

For nominal stems, the situation is a bit more complicated. Diakonoff (1988:59—61) reconstructs two "abstract" case forms for Proto-Afroasiatic: (a) **-i*/**-u* and (b) **-Ø*/**-a*. Diakonoff notes that the best preserved case marker was **-i*. It served two functions: (a) nominative-ergative and (b) genitive (in the sense 'belonging to'). In Cushitic, it often has two variants: (a) a short one in *-i* and (b) an "expanded" one in *-iya* or *-ii*. Given the identical form of the nominative-ergative and genitive, Diakonoff assumes that the nominative-ergative function arose from the genitive function. For **-Ø*/ **-a*, Diakonoff assumes that it represented "the noun outside of grammatical links (the so-called '*status indeterminatus*') or the noun-predicate (the so-called '*status praedicativus*'), but also the subject of a state or condition, including the subject of the state that resulted from the action." Finally, it should be noted that Sasse (1984:117) reconstructs the following two declensional paradigms for nouns with short final vowels for Proto-East Cushitic:

	Masculine	Feminine
Absolute Case	*-a	*-a
Subject Case	*-u/i	*-a

Sasse (1984) discusses the development of this system within Cushitic and ends by noting that traces of the above patterning can also be found in Semitic and Berber (cf. Proto-Semitic nominative **-u*, accusative **-a*, genitive **-i*).

I assume that the following patterning existed in early Proto-Nostratic:

1. **-i*/*u* was used to mark the subject (the agent) in active constructions — these subjects "perform, effect, instigate, and control events" (Mithun 1991:538);
2. **-a* was used to mark:

   (a) The direct object (the patient) of transitive verbs;
   (b) The subject ("non-agent subject" [= the patient]) in stative constructions — these subjects are "affected; things happen or have happened to them", just like direct objects (Mithun 1991:538);
   (c) The so-called "*status indeterminatus*".

In later Proto-Nostratic, this patterning became disrupted, though, as we have seen, it may have survived into Proto-Afroasiatic. In later Proto-Nostratic, the relational markers **-ma* and **-na* came to be used to mark the direct object of transitive verbs as well as the subject in stative constructions. Eventually, these relational markers became the primary means of marking the direct object of transitive verbs or the subject in stative constructions, with the result that the older patterning became disrupted. Thus, in the latest stage of the Nostratic parent language, we find the following patterning:

1. _*-i/u_: used to mark the subject in active constructions:

    (a) *CVC+i/u
    (b) *CVC+C_{DS}+i/u
    (c) *CVC-CVC+i/u

2. _*-a_ ~ _*-ma/*-na_: used to mark the direct object of transitive verbs as well as the subject in stative constructions:

    (a) *CVC+a plus _*-ma/*-na_: *CVC+a+ma/na
    (b) *CVC+C_{DS}+a plus _*-ma/*-na_: *CVC+C_{DS}+a+ma/na
    (c) *CVC-CVC+a plus _*-ma/*-na_: *CVC-CVC+a+ma/na

_*-ma/-na_ was the first case form (bound relational marker) to develop in Proto-Nostratic. The second was the genitive (in the sense 'belonging to') in _*-nu_. Indeed, these are the only two bound relational markers that can be confidently reconstructed for the latest period of Proto-Nostratic (see below for more information). Finally, it seems likely that unextended _*-a_ remained as the indicator of the _status indeterminatus_.

In Elamite, the _*-a_ (and _*-u_ ?) variant was eliminated in nominals. Dravidian, on the other hand, underwent further developments. Here, _*-i_ ~ _*-a_ were reinterpreted as oblique markers (on which, cf. Krishnamurti 2003:225—226), while _*-u_ assumed the role of enunciative vowel (cf. Krishnamurti 2003:91: "[w]hen roots in final obstruents are free forms, the consonant is geminated followed by a non-morphemic [enunciative] _u_.)".

This, then, explains the origin of both the so-called "formative vowels" and "terminal vowels". It may be noted here that Ehret (1995:15) concludes that the terminal vowels found in Afroasiatic "are fossils of a nominal morphology productive in pre-proto-Afroasiatic and predating the rise of grammatical gender in the family. Having lost their original grammatical function, they have been reanalyzed as markers of singular or sometimes, as in the case of Southern Cushitic, of the plural in nominals." As a further note, the terminal vowel _*-a_ may ultimately be the source of the highly productive thematic stems in later Proto-Indo-European.

Ehret does not reconstruct formative vowels for Proto-Afroasiatic. In this, he is correct. As noted above, in Proto-Afroasiatic, the earlier formative vowels have been reinterpreted as part of the derivational suffixes.

8. Rules of Proto-Nostratic Syntax

Dolgopolsky (1984:92—93 and 2005) sets up the following rules of Proto-Nostratic syntax:

> A. Words are classified into three groups (which differ in their syntactic behaviour):
>   a) Full Words (in the sense of the Chinese traditional grammar, i.e. semantic counterparts of nouns, adjectives, adverbs and verbs of modern languages),
>   b) Pronouns,
>   c) Grammatical Words (i.e. case-markers).
> B. Pronouns (if stressed) can behave syntactically according to the rules of Full Words as well.
> C. The predicate is the last Full Word of the sentence.
> D. Any object precedes its verb (i.e. its Full Word with verbal meaning).
> E. Any attribute (expressed by a Full Word) precedes its *regens*.
> F. A pronoun (personal or demonstrative) functioning as attribute follows its *regens*. In this case a personal pronoun has possessive meaning.
> G. A pronoun functioning as subject follows its predicate.
> H. Case-markers follow the corresponding Full Word. Some of these (genitive-marker **nu*, accusative-marker **ma*) follow immediately after its Full Word, while others (such as locative postpositions) can be used in a construction Full Word + **nu* + postposition. This accounts for **-n-* preceding the case-ending in the oblique cases of the IE heteroclita, for the increment **-iṉ-/ -n-* preceding the case endings of the oblique cases in D[ravidian], for some F[inno-] U[grian] case forms (locative **-na* < **nu Ha*), as well as for the **-n-*increment in the personal pronominal stems in the oblique cases (→ all cases) in U[ralic], T[urkic], T[ungusia]n, and D[ravidian]…
>
> A logical corollary of rules C—E is that the subject (if it is a Full Word) occupied the remaining place: somewhere in the initial part of the sentence.
>
> These rules have been preserved almost entirely (either as syntactic rules of word-order or as morpheme-order in grammatical forms) in Uralic, Turkic, Mongolian, Tungusian, Gilyak, Korean, Japanese, Dravidian, Early Indo-European, Cushitic, and have determined the order of morphemes within words in the rest of the Nostratic languages.

Proto-Nostratic syntax was head-final, or left-branching, that is, dependents preceded their heads according to the so-called "rectum-regens rule". In other words, "adverbs" preceded verbs, "adjectives" preceded nouns, and auxiliaries followed the main verb, though it must be emphasized here that adjectives did not exist as an independent grammatical category in Proto-Nostratic (see below for details). The unmarked syntactical order was Subject + Object + Verb (SOV).

From a typological perspective, the native American language Yuki of northern California (cf. Kroeber 1911) may be cited as an example of a language structurally similar to Proto-Nostratic. Hurrian (cf. Bush 1964; J. Friedrich 1969a; Laroche 1980; Speiser 1941; Wegner 1999 and 2007; Wilhelm 2004a) may be mentioned as another language that was structurally similar to Proto-Nostratic during the latest period of development, when bound morphemes had started to appear.

9. Pronominal, Deictic, and Anaphoric Stems

9.1. First Person Stems

First person singular (active): *_mi_
First person plural (inclusive, active): *_ma_
First person (stative): *_kʰa_
First person (stative): *_ħa_
First person singular: *_na_
First person plural (exclusive, active): *_na_
First person (postnominal possessive/preverbal agentive): *_ʔiya_

9.2. Second Person Stems

Second person: *_tʰi_, (oblique) *_tʰa_
Second person: *_si_
Second person: *_ni_

9.3. Anaphoric and Deictic Stems

Pronominal base of unclear deictic function: *_-gi_ (~*_-ge_)
Deictic particle: (A) *_ʔa-_ (~ *_ʔə-_) (distant), (B) *_ʔi-_ (~ *_ʔe-_) (proximate), and (C) *_ʔu-_ (~ *_ʔo-_) (intermediate)
Deictic particle: (A) *_kʰa-_ (~ *_kʰə-_) (proximate), (B) *_kʰu-_ (~ *_kʰo-_) (distant), and (C) *_kʰi-_ (~ *_kʰe-_) (intermediate)
Deictic particle: (A) *_tʰa-_ (~ *_tʰə-_) (proximate), (B) *_tʰu-_ (~ *_tʰo-_) (distant), and (C) *_tʰi-_ (~ *_tʰe-_) (intermediate)
Deictic particle: *_ša-_ (~ *_šə-_)
Anaphoric pronoun stem: *_si-_ (~ *_se-_)
Anaphoric pronoun stem: *_na-_, *_ni-_
Deictic particle: *_tʸʰa-_ 'that over there, that yonder (not very far)'

Note: The deictic particles (A) *_ʔa-_ (~ *_ʔə-_) (distant), (B) *_ʔi-_ (~ *_ʔe-_) (proximate), and (C) *_ʔu-_ (~ *_ʔo-_) (intermediate) often combined with other deictic stems.

9.4. Interrogative, Relative, and Indefinite Stems

Relative: *_kʷʰi-_ (~ *_kʷʰe-_); interrogative: *_kʷʰa-_ (~ *_kʷʰə-_)
Interrogative-relative stem: *_ʔay-_, *_ʔya-_
Interrogative: *_mi-_; relative: *_ma-_

Interrogative-relative: **na*
Indefinite: **ma-*, **mi-*, **mu-*
Indefinite: **dʸi-* (~ **dʸe-*) 'this one, that one'

9.5. Summary

The following two tables correlate the reconstructions for the Proto-Nostratic first and second person personal pronoun stems proposed in this paper (column A) with those proposed by Illič-Svityč (1971—1984; also V. Dybo 2004) (column B), Dolgopolsky (1984, 2005, and to appear) (column C), Greenberg (2000) (column D), and Kortlandt (2010a/b/c) (column E):

A. First person personal pronouns:

	A	B	C	D	E
1st pers. sg. (active)	**mi*	**mi*	**mi*	**m*	**mi*
1st pers. pl. (incl., active)	**ma*	**mä*		**m*	**me*
1st pers. (stative)	**kʰa*			**k*	
1st pers. (stative)	**ħa*				
1st pers. sg.	**na*	**naHe-na*, **na*		**n*	
1st pers. pl. (excl., active)	**na*		**n̄*	**n*	
1st pers. (postnominal)	**ʔiya*		**Hoy*		

B. Second person personal pronouns:

	A	B	C	D	E
2nd pers.	*tʰi, *tʰa	*ṭʌ-na, *ṭa	*ṭ[ü] (> *ṭi)	*t	*te
2nd pers.	*si	*si- possessive	*ś[ü] (> *śi)	*s	
2nd pers.	*ni			*n	

This table correlates the reconstructions for the Proto-Nostratic anaphoric, deictic, interrogative, relative, and indefinite stems proposed in this paper (A) with those proposed by Illič-Svityč (B), Dolgopolsky (C), Greenberg (D), and Kortlandt (E):

	A	B	C	D	E
Deictic particle	*-gi (~ *-ge)			*ge	
Deictic particle	*ʔa- (~ *ʔə-), *ʔi- (~ *ʔe-), *ʔu- (~ *ʔo-)	*ʔa, *ʔi/*ʔe	*ha, *[h]e, *[h]i, *[h]u	*i ~ *e, *a ~ *e	*i/*e
Deictic particle	*kʰa- (~ *kʰə-), *kʰu- (~ *kʰo-), *kʰi- (~ *kʰe-)		*Ḳ[ü]	*ku	
Deictic particle	*tʰa- (~ *tʰə-), *tʰu- (~ *tʰo-), *tʰi- (~ *tʰe-)	*ṭa	*ṭä	*t	*t
Deictic particle	*ša- (~ *šə-)			*s	*s
Anaphoric stem	*si- (~ *se-)	*šä	*sE		
Anaphoric stem	*na-, *ni-		*nE (dual)		
Deictic particle	*tʸʰa-		*ćE		
Relative	*kʷʰi- (~ *kʷʰe-)				
Interrogative	*kʷʰa- (~ *kʷʰə-)	*ḳo	*Ḳo	*k	*k
Interrogative-relative	*ʔay-, *ʔya-	*ja	*ya	*j	
Interrogative	*mi-	*mi	*mi	*m	
Relative	*ma-				
Interrogative-relative	*na-	*na		*n	
Indefinite	*ma-, *mi-, *mu-	*mu			
Indefinite	*dʸi- (~ *dʸe-)				

10. Nominal Morphology

10.1. Introduction

The overall structure of nominals (nouns and adjectives) was as follows:

Root (+ derivational suffix) + terminal vowel
(+ relational marker) (+ number marker)

A stem could consist of the unextended root (**CVC-*) or the root extended by a single derivational suffix (**CVC+C-*). As noted above, it is necessary to recognize two distinct periods of development in Proto-Nostratic. In the earliest phase of development, the relational markers listed below were free relational morphemes (postpositional particles). In later Proto-Nostratic, however, at least two of them were well on their way to becoming bound relational morphemes (case suffixes).

As just stated, only the following two bound relational markers (case suffixes) can be confidently reconstructed for the latest period of Proto-Nostratic: (a) direct object **-ma*, **-na* and (b) genitive **-nu*. Other case relationships were expressed by postpositions (see below for a list), some of which developed into bound case morphemes in the individual daughter languages. This is confirmed by Dravidian, where only the accusative (**-ay*, **-Vn*), dative (**-kk-*/**-k-*), and genitive (**-a*, **-in*) can be confidently reconstructed for the Dravidian parent language (cf. Krishnamurti 2003:227; Steever 1998:20 [Steever adds nominative **-Ø*]). Other case forms developed within the Dravidian daughter languages (for discussion, cf. Krishnamurti 2003:227—243). Likewise, only the following two grammatical cases can be reconstructed for Proto-Uralic (cf. Abondolo 1998:18; Raun 1988:558—559): (a) accusative **-m*, which probably was used to mark the definite direct object of finite verbs, and (b) a subordinate suffix **-n*, which functioned as a genitive/nominalizer with nouns and as an adverb formant with verbs. Abondolo (1998:18) further points out that there were also at least three local cases in Proto-Uralic: (a) locative **-nA*, (b) separative **-tA* ~ **-tI*, and (c) and perhaps the latives **-k* (and/or **-ŋ*) and **-t*^{y} (traditional **-ć*) (and/or **-n*^{y} [traditional **-ń*]). Sinor (1988:714—725) devotes considerable attention to the question of common case markers between Uralic and Altaic. He, too, posits a Proto-Uralic accusative in **-m* and a genitive in **-n*. For the former, he notes that nothing comparable can be posited for Proto-Turkic or Proto-Mongolian, but he does reconstruct a Proto-Tungus accusative **-m*, which is in agreement with what is found in Uralic. The clearest parallels for the latter are to be found in the Proto-Mongolian genitive **-n* (cf. Poppe 1955:187—194) and in the Proto-Turkic genitive **-n* (cf. Róna-Tas 1998:73). Poppe (1955:187—194) mentions that the genitive and accusative have converged in some Mongolian languages. This seems to indicate that Proto-Mongolian may have preserved the **-n* variant accusative form as opposed to the **-m* variant found in

Uralic and Tungus. Sinor (1988:715—725) also discusses the Uralic and Altaic parallels between various local cases. Finally, it is worth mentioning here that, within Afroasiatic, Zaborski (1990:628) tentatively reconstructs the following case morphemes for Proto-Omotic: (a) nominative **-i*, (b) genitive-instrumental-directional **-kV*, (c) dative **-s*, (d) dative-comitative **-rV*, (e) accusative **-a* and **-nV*, (f) instrumental-locative-directional-dative **-nV*, and (g) ablative **-pV*. Zaborski (1990:618) notes that some of these case forms may go back to earlier postpositions. Parallels with Cushitic show that at least some of these case forms go back to Proto-Afroasiatic. Diakonoff (1988:61) notes that the following cases can be established for Proto-Afroasiatic with reasonable certainty: (a) **-Vš*, **-šV* locative-terminative; (b) **-dV*, **-Vd* comitative, dative; (c) **-kV* ablative and comparative; (d) **-Vm* locative-adverbialis; (e) **-l* directive; and (f) **-p* (also **-f*) ablative (in Omotic); conjunction, demonstrative pronoun in other languages. The ultimate Nostratic origin of several of the case forms posited by Zaborski for Proto-Omotic and by Diakonoff for Proto-Afroasiatic is completely transparent.

In Proto-Nostratic, adjectives did not exist as a separate grammatical category. They were differentiated from nouns mainly by syntactical means — a noun placed before another noun functioned as an attribute to the latter. Moreover, they did not agree with the head noun in number or gender. Caldwell (1913:308—318) describes similar patterning for Dravidian: "...adjectives have neither number, gender, nor case, but are mere nouns of relation or quality, which are prefixed without alternation to substantive nouns". Krishnamurti (2003:389) points out, however, that not all Dravidian adjectives are of the derived types described by Caldwell. Krishnamurti considers adjectives to form a separate part of speech in Dravidian, as does Zvelebil (1977:59—69 and 1990:27—28), though Zvelebil mentions the fact that primary, underived adjective stems are statistically very rare in the Dravidian daughter languages. According to Steever (1998:19): "The reconstruction of further parts of speech such as adjectives and adverbs to the proto-language is controversial. While some scholars have projected the category of adjectives to Proto-Dravidian, many of the candidates for adjectival status appear to be defective nouns or verbs. Although the scholarly literature speaks of certain forms as having adjectival function, viz., modifying a nominal, conclusive evidence that those forms constitute a formally distinct class is largely lacking. Further, none of the putative adjectives in Dravidian exhibits a comparative or superlative degree. These degrees are expressed instead by syntactic means..." As for Elamite, Khačikjan (1998:17) notes: "There was no special class of adjectives in Elamite. The mechanism of forming adjectives was the same as that used to express attributive relationships." According to Diakonoff (1988:57), adjectives did not form a separate grammatical category in Proto-Afroasiatic, and this appears to have been the situation in Proto-Berber (cf. Kossmann 2012:34) and probably Proto-Cushitic (cf. Mous 2012:359) as well. Likewise in Proto-Uralic (cf. Abondolo 1998:18): "Nouns were probably not morphologically distinct from adjectives in proto-Uralic, although the distribution of the comparative suffix *=mpV suggests that an adjective category may have been developing before the breakup of Finno-Ugric". In later Proto-Indo-European, on the other hand, adjectives formed a distinct grammatical category, and they agreed with the head noun in number and gender (for details and examples, cf. Szemerényi 1996:192—201 and Beekes 1995:196—200). Adjectives also form a separate part of speech in the Kartvelian languages. In Turkic, adjectives are not usually clearly distinguished from nouns morphologically. However, several suffixes are used primarily to form adjectives. In Modern Mongolian, there is no difference between adjectives and nouns. A noun placed before another noun functions as an attribute to the latter. In Gilyak / Nivkh, adjectives do not exist as a distinct word-class, the semantic function of adjectives being performed by qualitative verbs.

10.2. Relational Markers

Direct object: *-*ma*
Direct object: *-*na*
Possessive: *-*nu* 'belonging to'
Possessive: *-*lV* 'belonging to'
Dative: *-*na* 'to, for'
Directive: *-$k^{h}a$ 'direction to or towards, motion to or towards'
Directive(-locative): *-*ri* 'direction to or towards, motion to or towards' (?)
Locative: *-*ni* 'the place in, on, or at which something exists or occurs'
Locative, instrumental-comitative: *-*ma* 'in, from, with'
Locative: *-*bi* 'in addition to, together with'
Locative: *-*i* 'near to, near by' (?)
Comitative-locative: *-*da* 'together with'
Oblique: *-$t^{h}a$

The following table correlates the reconstructions for the Proto-Nostratic relational markers proposed in this paper (A) with those proposed by Illič-Svityč (B), Dolgopolsky (C), Greenberg (D), and Kortlandt (E):

	A	B	C	D	E
Direct object	*-*ma*	*-*mʌ*	*-*mA*	*-*m*	*-*m*
Direct object	*-*na*				
Possessive	*-*nu*	*-*n*	*-*nu*	*-*n*	*-*n*
Possessive	*-*lV*			*-*l*	
Dative	*-*na*				*-*nV*
Directive	*-$k^{h}a$	*-*ḳʌ*	*-*Ḳ* [= *-*ḳ* ?]	*-*ka* Dative	*-*ka* Dative

	A	B	C	D	E
Directive(-locative)	_*-ri_			_*-ru_	_*-rV_
Locative	_*-ni_	_*-na_		_*-n_	_*-nV_
Locative, instr.-comit.	_*-ma_			_*-m_	
Locative	_*-bi_			_*-bh-_	
Locative	_*-i_			_*-i_	
Comitative-locative	_*-da_	_*-da_ Loc.	_*-d[E]H$_1$a_	_*-ta_ Locative	_*-du_, _*-da_ (Altaic)
Oblique	_*-t^{h}a_	_*-tʌ_ Instr.		_*-ta_ Ablative	_*-t_ Ablative

10.3. Dual and Plural Markers

Dual: _*k^{h}i(-nV)_
Plural: _*-t^{h}a_
Plural: _*-ri_
Plural: _*-k^{h}u_
Plural (Eurasiatic only): _*-sV_
Plural/collective: _*-la_
Plural: _*-nV_

The following table correlates the reconstructions for the Proto-Nostratic dual and plural markers proposed in this paper (A) with those proposed by Illič-Svityč (B), Dolgopolsky (C), Greenberg (D), and Kortlandt (E):

	A	B	C	D	E
Dual	_*k^{h}i(-nV)_		_*-q̇_	_*ki[n]_	_*-ki_
Plural	_*-t^{h}a_	_*-t_	_*-t_	_*-t_	_*-t_
Plural	_*-ri_		_*-r[i]_	_*-ri_	
Plural	_*-k^{h}u_		_*-kU_	_*-ku_	
Plural (Eurasiatic only)	_*-sV_			_*-s_	
Plural/collective	_*-la_	_*-lA_	_*-l̦A_	_*-l_	
Plural	_*-nV_	_*-nA_	_*-n[ä]_	_*-n_	

10.4. Derivational Suffixes

Nominalizer: _*-r-_
Nominalizer: _*-m-_
Nominalizer: _*-y-_
Nominalizer: _*-t^h-_

Nominalizer: **-n-*
Nominalizer: **-l-*
Nominalizer: *-k^h-
Nominalizer: **-k'-*

Note: No doubt, there were additional derivational suffixes in Proto-Nostratic. Indeed, it appears that any consonant could serve as a derivational suffix. Ehret (1995:15—54) lists and discusses a great variety of nominal and verbal extensions in Afroasiatic, while Starostin—Dybo—Mudrak (2003:173—220) do the same for Altaic. For a comprehensive treatment of Indo-European derivational morphology, cf. Brugmann 1904:281—354, and for Uralic, cf. Collinder 1960:255—281 and Décsy 1990:58—66.

10.5. Noun Morphology in the Daughter Languages

According to John C. Kerns (Bomhard—Kerns 1994:172—173, §3.5.3), Proto-Nostratic may have had three nominal declensions: (A) the *first declension*, corresponding to the neuter heteroclitic declension in Indo-European; (B) the *second declension*, corresponding to the other neuter paradigms in Indo-European, and (C) the *third declension*, a variation of the second wherein a definite-accusative singular was marked by the termination **-m*. Kerns states that the accusative had no special marker in the first two declensional types. He also notes that the accusative singular ending **-m* is found in Proto-Uralic and is also widely-represented in Dravidian languages for the full set of Proto-Dravidian case endings, see below). The views expressed by Kerns differ from those presented in the present paper, where Proto-Nostratic is seen as a language of the active type.

Kerns reconstructs the following singular case endings for Common Uralic:

Nominative	*-Ø
Accusative	*-m
Genitive	*-n
Dative-Lative	*-n^{y}V (palatalized **-n* followed by a front
Locative	*-na
Ablative	*-ta and *-δa

Kerns believes that the above endings, "with a few reservations", can also be attributed to Proto-Nostratic (here, I would substitute "Proto-Eurasiatic" for his "Proto-Nostratic" — Kerns himself uses "Eurasiatic" in his 1985 book *Indo-European Prehistory*). According to Abondolo (1998:18), there were at least two grammatical cases in Proto-Uralic: an accusative **-m* and a subordinate suffix **-n*, which functioned as a genitive/pronominalizer. There were at least three local cases as well: a locative **-nA*, a separative **tA* ~ **tI*, and perhaps the latives **-k* (and/or **-ŋ*) and *-t^y (and/or *n^y).

At this point, it is interesting to compare the case endings (properly, tightly bound postpositions) reconstructed for Proto-Dravidian by Zvelebil (1977:33):

Nominative	*-*Ø* and, possibly, *-*m*/*-*n* with non-personal substantives
Accusative	*-*(V)n*
Genitive	*-*in* (adnominal); *-*atu* (pronominal); *-*ā̆* (possessive)
Dative	*-*(k)ku*
Instrumental	*-*ān*/**āl*
Ablative	*-*in* (?)
Locative	*-*uḷ*; *-*in*/*-*il* (?); *-*kaṇ*
Sociative (Comitative)	*-*ō̆ṭu* or *-*(t)-ō̆ṭu* < **tō̆ṛV* (?)

This system can be derived from an earlier, simpler system, as is shown by comparison with Elamite (cf. McAlpin 1981:108—112). Clearly, several of the endings must have had a common origin (such as the genitive ending *-*in*, the ablative *-*in*, and the locative *-*in*[/*-*il*]). McAlpin (1981:111) reconstructs the following case endings for Proto-Elamo-Dravidian:

Nominative	*-Ø
Accusative	*-(V)n
Adessive/ Purposive (Dative)	*-əkkə (?)
Genitives:	
1. Possessive	*-a
2. Adnominal	*-in
3.	*-tə

According to Ramstedt (1952—1957.I:25—27) and Poppe (1955:187—191), a genitive in *-*n* also existed in Proto-Altaic. This ending is still found in several Mongolian and Turkic languages, though the Turkic forms vary between -*n* and -*ŋ*. However, Sinor (1988:715) cautions that it is premature to assume a Common Altaic genitive in *-*n*.

To fill out the picture, let us look at the case endings traditionally reconstructed for Late Proto-Indo-European, that is, for the stage of development immediately prior to the emergence of the non-Anatolian Indo-European daughter languages (cf. Brugmann 1904:373—399; Watkins 1998:65—66; Fortson 2004:113; Szemerényi 1996:157—192; Beekes 1995:172—195; Schmitt-Brandt 1998:180—220; Meier-Brügger 2003:195—199; Haudry 1979:34—37 and 1982; Meillet 1964:292—300; Lehmann 1993:144—146;

Clackson 2007:92—100; Schmalstieg 1980:46—87; Shields 1982; Adrados 1975.I:329; Adrados—Bernabé—Mendoza 1995—1998. II:45—94; Burrow 1973:230—242; Buck 1933:172—208; Sihler 1995:248—256; Rix 1992:117—119) (the following table is a composite from multiple sources and aims to be as comprehensive as possible; some of the reconstructions are more certain than others):

Case	Singular	Plural	Dual
Nominative	*-s, *-Ø	*-es	*-e, *-ī/*-i
Vocative	*-Ø	*-es	
Accusative	*-m/*-m̥	*-ns/*-n̥s	
Genitive	*-es/*-os/*-s	*-om/*-ōm	*-ous (?), *-ōs (?)
Ablative	*-es/*-os/*-s; *-ed/*-od	*-bh(y)os, *-mos	*-bhyō, *-mō
Dative	*-ei	*-bh(y)os, *-mos	*-bhyō, *-mō
Locative	*-i	*-su	*-ou
Instrumental	*-e/*-o; *-bhi, *-mi	*-ōis; *-bhis, *-mis	*-bhyō, *-mō

Missing from this table is the thematic nominative-accusative neuter singular ending **-m* — this form is to be derived from the accusative singular ending. The *-b^h- and *-*m*- endings found in several of the concrete cases are usually considered to be late additions, and some have even questioned whether or not they should even be posited for the Indo-European parent language. They are not found in Hittite. No doubt, these endings were originally adverbs that were gradually incorporated into the case system, with some daughter languages choosing *-b^h- and others choosing *-*m*-. They should not be reconstructed as case endings at the Proto-Indo-European level. In like manner, the genitive plural probably arose from the accusative singular, while the genitive singular and nominative singular endings in *-*s* must have had a common origin — these endings later spread from the genitive singular to the ablative singular. The dual was a late addition, while the plural originally had a reduced set of endings compared to what was found in the singular — this is the picture that emerges when the Hittite and other Anatolian data are brought into consideration. We may note here that the Proto-Uralic ablative ending *-*ta* and the Proto-Elamo-Dravidian oblique/locative ending *-*tə* are most probably related to the Indo-European ablative *-et^h/*-ot^h (given as *-*ed*/*-*od* in the above table — the phonetics are uncertain here).

In his book *Indo-European Prehistory*, John C. Kerns (1985:109—111) devotes considerable attention to describing an oblique-*n* marker, which he claims is a major component in Indo-European heteroclitic stems, and he elaborates upon his ideas in his treatment of Nostratic declension in Bomhard—Kerns (1994:173—179, §3.5.3.1). He notes that this oblique-*n* is the source of the -*n* found in the genitive, ablative, and instrumental case endings in Dravidian — it is also found in the genitive, dative-lative (palatalized before a front vowel), and locative case endings in Uralic. Kerns even finds traces of this oblique-*n* in Eskimo and Japanese. Thus, this is a widespread and ancient feature. Greenberg (2000:130) also discusses this ending (see also Cavoto 1998:26):

> There is an *-n* genitive in Eurasiatic that frequently serves as a marker of the oblique case along with more specific indicators of location, instrument, etc. When this occurs it invariably precedes the specific indicator. In certain cases it has also spread to the nominative.

11. Verbal Morphology

11.1. Introduction

In Proto-Nostratic, verbs fell into two types of construction: (1) active and (2) stative. In active constructions, which usually involved transitive verbs, the grammatical subject of the verb represented the agent performing the action, and the direct object represented the patient, or recipient, of the action (cf. Trask 1993:5). Stative constructions, on the other hand, expressed a state of affairs, rather than an event (cf. Trask 1993:259). Verbs expressed aspectual contrasts rather than temporal contrasts. Tense relates the time of the situation referred to to some other time, usually to the moment of speaking (cf. Comrie 1976:1—2), while aspect marks the duration or type of temporal activity denoted by the verb (cf. Crystal 1992:29; Comrie 1976:3). Proto-Nostratic had two aspects: (a) perfective (past) and (b) imperfective (non-past). Here, we may note that Diakonoff (1988:85) posits two aspects for the earliest form of Proto-Afroasiatic: (a) punctive (instantaneous) and (b) durative (protracted, or continuous). He assumes that these later developed into perfective and imperfective aspects and then, eventually, in the individual Afroasiatic daughter languages, into past and present-future tenses. He does not posit tenses for the Afroasiatic parent language. Proto-Nostratic also had, at the very least, the following moods: (a) indicative; (b) imperative; (c) conditional; (d) inchoative; (e) hortatory-precative; and (f) prohibitive. There was also a causative construction.

The overall structure of verbs was as follows:

Root + formative vowel (+ derivational suffix)
(+ mood marker) (+ person marker) (+ number marker)

A stem could consist of the unextended root or the root extended by a single derivational suffix (preceded, as indicated above, by a formative vowel). The position of the number marker seems to have been flexible — it could also be placed before the person marker. Gender was not marked. There were no prefixes in Proto-Nostratic. We may note here that Krishnamurti (2003:279 and 312) posits the following structure for verbs in Proto-Dravidian:

Stem + tense-mood + (gender-)number-person marker

Stative verbs were indifferent to number and, therefore, had no plural forms. They also had a special set of person markers different from those of active verbs:

	Active		Stative
Person	Singular	Plural	Singular only
1	*-mi *-na	*-ma (inclusive) (+ plural marker) *-na (exclusive) (+ plural marker)	*k^{h}a *ħa
2	*-t^{h}i *-si *-ni	*-t^{h}i (+ plural marker)	*t^{h}i
3	*ša- (~ *šə-) *na-, *ni-	*ša- (~ *šə-) (+ plural marker) *na-, *ni- (+ plural marker)	*Ø

Morphologically, verbs could be either finite or non-finite. Finite forms could be marked for aspect, mood, person, and number, but not for gender or tense. Non-finite forms exhibited nominal inflection. In unmarked word order, the verb occupied the end position of a clause.

11.2. Non-finite Verb Forms

The following non-finite verb forms are widespread enough in the Nostratic daughter languages to guarantee their common origin, and, consequently, they are listed separately here. However, at the Proto-Nostratic level, they were indistinguishable from the nominalizing suffixes listed above.

Participle: **-n-*
Participle: **-t^{h}-*
Gerundive-participle: **-l-*

The following table correlates the reconstructions for the Proto-Nostratic non-finite verb forms proposed in this paper (A) with those proposed by Illič-Svityč (B), Dolgopolsky (C), Greenberg (D), and Kortlandt (E):

	A	B	C	D	E
Participle	_*-n-_		_*n̄_	_*n_	_*n_
Participle	_*-t^h-_		_*ṭ_	_*t_	_*t_
Gerundive-participle	_*-l-_			_*l_	_*l_

Note: Greenberg (2000:182—186, no. 44) also posits a participle in _*-nt-_ for Proto-Eurasiatic on the basis of reflexes found in Indo-European, Finno-Ugrian, and Gilyak / Nivkh. However, this is best seen as a compound suffix: _*-n-_ + _*-t^h-_.

11.3. Finite Verb Forms: Mood Markers

Indicative: unmarked
Imperative: _*-k^{h}a_, _*-k^{h}i_, _*-k^{h}u_; _*-a_, _*-i_, _*-u_
Conditional: _*-ba_
Hortatory-precative: _*-li_
Inchoative: _*-na_

Note: The bare stem could also serve as imperative, in which case the vowels _*-a_, _*-i_, or _*-u_ were added to the stem. These were different than the formative vowels (aspect markers) previously discussed. Ultimately, they may go back to the deictic particles (A) _*ʔa-_ (~ _*ʔə-_) (distant), (B) _*ʔi-_ (~ _*ʔe-_) (proximate), and (C) _*ʔu-_ (~ _*ʔo-_) (intermediate).

The following table correlates the reconstructions for the Proto-Nostratic mood markers proposed in this paper (A) with those proposed by Illič-Svityč (B), Dolgopolsky (C), Greenberg (D), and Kortlandt (E):

	A	B	C	D	E
Imperative	_*-k^{h}a_, _*-k^{h}i_, _*-k^{h}u_		_*k_ ~ _*g_	_*ka_	
Conditional	_*-ba_			_*p_	
Hortatory-precative	_*-li_			_*l_	
Inchoative	_*-na_				

11.4. Finite Verb Forms: Others

Causative: _*-sV_

The following table correlates the reconstruction for the Proto-Nostratic causative marker proposed in this paper (A) with that proposed by Illič-Svityč (B), Dolgopolsky (C), Greenberg (D), and Kortlandt (E):

	A	B	C	D	E
Causative	**-sV*			**s*	

11.5. Verb Morphology in the Daughter Languages

Comparison of the various Nostratic daughter languages reveals many striking similarities in verb morphology. This comparison, for example, allows us to ascertain the ultimate origin of the athematic verb endings in Proto-Indo-European: they can be nothing other than possessive suffixes similar to what are found in Proto-Uralic and Proto-Altaic. Ultimately, these possessive suffixes had a pronominal origin. The earliest forms of the athematic endings in Proto-Indo-European may have been as follows (cf. Bomhard 1988; see also Villar 1991:244—252; for details, cf. Chapters 18 and 19 of my 2008 book):

Person	Singular	Plural
1	*-m	*-me
2	*-t^h	*-t^he
3	*-s, *-Ø	*-en

This earlier system may be partially preserved in Tocharian A, where the athematic endings are as follows:

Person	Singular	Plural
1	-(ä)m	-mäs
2	-(ä)t	-c
3	-(ä)ṣ	-(i)ñc

Note: There are phonological problems with the 3rd singular ending *-(ä)ṣ* in Tocharian — had this been inherited directly from Proto-Indo-European **-si*, we would expect *-(ä)s*, not *-(ä)ṣ*. The best explanation is that of Pedersen, who derived this ending from an enclitic **se-*. For details on the develop-ment of the personal endings in Tocharian, cf. Adams 1988:51—62; Van Windekens 1944:297—321 and 1976—1982.II/2:259—297.

Traces of the earlier system are also found in the Anatolian languages. Note, for example, the Hittite 2nd singular active preterite ending *-ta*.

Now compare the following system of personal endings, which are assumed to have existed in Proto-Uralic (cf. Hajdú 1972:40 and 43—45; Cavoto 1998:127; Collinder 1965:134—135; Décsy 1990:66—68; Sinor 1988:725):

Person	Singular	Plural
1	*-me	*-me (+ Plural)
2	*-te	*-te (+ Plural)
3	*-se	*-se (+ Plural)

In an unpublished paper entitled "Cross-Bering Comparisons", Stefan Georg lists the following possessor suffixes in "Uralo-Eskimo", Samoyed, and Eskimo-Aleut:

	Uralo-Eskimo		Samoyed		Eskimo-Aleut	
	Singular	Plural	Singular	Plural	Singular	Plural
1sg	-m	-t-m	-mə	-t-mə	-m-(ka)	-t-m-(ka)
2sg	-t	-t-t	-tə	-t-tə	-n/t	-tə-n/t
3sg	-sa	-i-sa	-sa	-i-sa	-sa	-i-sa
1pl	-mə-t	-n/t-mə-t	-ma-t	-t/n-ma-t	-mə-t	(= sg.)
2pl	-tə-t	-t-mə-t	-ta-t	-t-ta-t	-tə-t	(= sg.)
3pl	-sa-t	-i-sa-t	-i-to-n	-to-n	-sa-t	-i-sa-t

The personal endings survive in Elamite as well, especially in the 2nd and 3rd persons (by the way, the Elamite 1st singular ending, *-h*, is, of course, related to the 1st singular perfect ending *-*ə*$_2$*e* of traditional Proto-Indo-European, which is found, for example, in Luwian in the 1st singular preterite ending *-ḫa*, in Hittite in the 1st singular ending *-ḫi*, and in Greek in the 1st singular perfect ending -α; this ending may also be related to the Proto-Kartvelian 1st person personal prefix of the subject series, **xw-*) — compare, for example, the conjugation of *hutta-* 'to do, to make' from Middle Elamite (cf. Reiner 1969:76; Grillot-Susini 1987:33):

Person	Singular	Plural
1	hutta-h	hutta-hu (< -h+h)
2	hutta-t	hutta-ht (< -h+t)
3	hutta-š	hutta-hš (< -h+š)

Traces of the 2nd singular ending are also found in Dravidian — McAlpin (1981:120) reconstructs Proto-Elamo-Dravidian 2nd person ending **-ti* (> Proto-Elamite **-tə*, Proto-Dravidian **-ti*). This is a significant archaism, since it bears no apparent resemblance to the common Elamo-Dravidian 2nd person personal pronoun stem, which McAlpin (1981:114—115) reconstructs as **ni* and which may be an innovation, though

Greenberg (2000:76—77) discusses the possibility that there may have been a second person pronoun stem **nV* in Eurasiatic.

Traces of these endings are found in the Altaic languages as well. Sinor (1988:725) reconstructs the following possessive suffixes for Proto-Turkic and Proto-Tungus:

Proto-Turkic:

Person	Singular	Plural
1	*-m	*-m (+ Plural)
2	*-ŋ	*-ŋ (+ Plural)
3	*-s	*Ø

Proto-Tungus:

Person	Singular	Plural
1	*-m	*-m (+ Plural)
2	*-t	*-t
3	*-n	*-t

It may be noted here that Common Mongolian did not have special verbal endings to indicate person or number. However, at a later date, personal pronouns were added enclitically to the verbal forms (cf. Poppe 1955:251).

Traces of these endings are also found within Afroasiatic in Highland East Cushitic, where the suffixes of the simple perfect in Gedeo / Darasa, Hadiyya, Kambata, and Sidamo are as follows (cf. Hudson 1976:263—264):

Person	Gedeo / Darasa	Hadiyya	Kambata	Sidamo
1 sg.	-enne	-ummo	-oommi	-ummo
2 sg.	-tette	-titto	-toonti	-itto
3 sg. m.	-e	-ukko	-o(ˀi)	-í
3 sg. f.	-te	-toˀo	-too(ˀi)	-tú
3 sg. pol.	—	-aakkoˀo	-semma(ˀi)	-ní
1 pl.	-nenne	-nummo	-moommi	-nummo
2 pl.	-tine	-takkoˀo	-teenta(ˀi)	-tiní
3 pl.	-ne	-toˀo	-too(ˀi)	-tú

While the suffixes of the present perfect in Hadiyya, Kambata, and Sidamo were as follows (cf. Hudson 1976:264—265):

Person	Hadiyya	Kambata	Sidamo
1 sg.	-aammo	-eemmi	-oommo
2 sg.	-taatto	-tenti	-otto
3 sg. m.	-aakko	-eeˀi	-inó
3 sg. f.	-taˀokko	-teeˀi	-tinó
3 sg. pol.	-aakkaˀokko	-eemma(ˀi)	-noonni
Person	Hadiyya	Kambata	Sidamo
1 pl.	-naammo	-neemmi	-noommo
2 pl.	-takkaˀokko	-teenta	-tinonni
3 pl.	-taˀokko	-teeˀi	-tinó

The suffixes of the imperfect are as follows (cf. Hudson 1976:265):

Person	Gedeo /	Hadiyya	Kambata	Sidamo
1 sg.	-anno	-oommo	-aammi	-eemmo
2 sg.	-tatto	-tootto	-taanti	-atto
3 sg. m.	-aani	-ookko	-ano	-anno
3 sg. f.	-taani	-tamo	-taaˀi	-tanno
3 sg. pol.	—	-aakkamo	-eenno	-nanni
1 pl.	-nanno	-noommo	-naammi	-neemmo
2 pl.	-tinaa	-takkamo	-teenanta	-tinanni
3 pl.	-naani	-tamo	-taaˀi	-tanno

The suffixes of the subordinate conjugation in Kambata and Sidamo are as follows (cf. Hudson 1976:270):

Person	Kambata	Sidamo
1 sg.	-a	-a
2 sg.	-ta	-ta
3 sg. m.	-a	-a
3 sg. f.	-ta	-ta
3 sg. pol.	-eena	-na
1 pl.	-na	-na
2 pl.	-teena	-tina
3 pl.	-ta	-ta

According to Ehret (1980:65), in Southern Cushitic, "[t]he basic person marking was constructed of the verb stem plus suffixes of the two shapes -V and -VCV, as the following comparison of West Rift and Dahalo conjugations indicates:"

Person	Proto-SC	Burunge	Iraq̱w	Dahalo
1 sg.	*-o	-Ø	-Ø	-o
2 sg.	*-ito	-id	underlying *-it	-Vṯo
3 sg. m.	*-i	-i	underlying *-i	-i
3 sg. f.	*-ito	-id	*-t	-Vṯo
1 pl.	*-anu	-an	-an	-Vnu
2 pl.	*-ite	-idey	underlying *-ta	-Vṯe
3 pl.	*-eye and *-iye	-ey, -i	underlying *-iya, also -ir	-ee

Finally, Bender (2000:202) lists the following verbal affixes in the *ta/ne* (TN) branch of Omotic:

Person	NWO	SE	C'	MO	G	Y	K	TN
1 sg.	*n; a	t(i)	e ?	*n ~	u	an; ut	*n; *e	—
2 sg.	*-; a	n(i)	a ?	*a	u/en	at+á;	*i(n)	—
3 sg.	*-; *i	(e)s	e ?	*e ~	u	é; na	*é	*e
3 sg. f.	*u; a	is	—		u	à	*a	*a
1 pl.	*n; i	uni	i ?	*ni	u	ni	*o/u(n	*un
2 pl.	*et+i;	t ~	i ?	*ti	end	eti	*ot;	*eti
3 pl.	*on+a;	usi	i ?	*i	end	son+e	*et;	*on

Abbreviations: NWO = Northwest Ometo; SEO = Southeast Ometo; C' = C'ara; MO = Macro-Ometo; G = Bench / Gimira; Y = Yemsa / Janjero; K = Kefoid; TN = *ta/ne* branch of Omotic.

The 1st person possessive suffix in **-m* was thus common to Indo-European, part of Afroasiatic (Highland East Cushitic), Uralic, and, within Altaic, Turkic and Tungus, while the 2nd person in **-t* was common to Indo-European, Uralic, Tungus, Elamo-Dravidian, and Afroasiatic, and the 3rd person in **-s* was common to Indo-European, Uralic, Turkic, Elamite, and Kartvelian (cf. Old Georgian *c'er-s* 'writes'). The 3rd singular possessive suffix was **-n* in Proto-Tungus, and this mirrors what is found in the 3rd plural in Indo-European and Kartvelian (cf. Old Georgian 3rd plural suffix *-en* in, for example, *c'er-en* 'they write', Mingrelian 3rd plural suffix *-an*, *-a*, *-n*, Laz 3rd plural suffix *–an*, *-n*), in Berber (cf. Kossmann 2012:44—47) and Beja / Beḍawye (cf. Appleyard 2007a:467), and partially in the 3rd singular and plural suffixes and Highland East Cushitic, with traces in Omotic (see above) and perhaps Semitic (R. Stempel [1999:105—106] takes the 3rd plural froms in **-n(a)* to be late formations taken over from the 2nd plural, while Moscati [1964:140] suggests that they are due to analogy with certain personal pronouns) — there is also a parallel here in Sumerian (cf. Bomhard 2008.I:269—271). As noted by Fortescue (1998:99), it is also found in Chukchi-Kamchatkan:

> Although, as we have seen, C[hukchi-]K[amchatkan] does not have personal possessor affixes of the E[skimo-]A[leut] type, it seems that there are traces of a 3rd person possessor marker remaining, of the same type found in Yukaghir before case endings (to be discussed in 5.1.2). Thus the 3rd person marker -(ə)n is frozen into position following the stem in the 'Class 2' noun declension for definite, individualized persons (in Chukchi mainly proper names, elder kinship terms and some other animates, including nicknames for domestic reindeer and names of animals in myths).

Within Indo-European, the 2nd singular ending **-tʰ* is preserved in Hittite and Tocharian. This was later replaced by what had been the 3rd singular, namely, **-s*. In his 1962 book entitled *Indo-European Origins of the Celtic Verb. I: The Sigmatic Aorist*, Calvert Watkins discusses the extensive evidence from the Indo-European daughter languages for an original 3rd singular ending in **-s*. It was Watkins who also showed that the 3rd singular indicative was originally characterized by the fundamental ending *zero*. The **-n-* found in the 3rd plural was a relic of the 3rd person ending found in Tungus, Kartvelian, and Sumerian. The development of the 3rd singular ending **-tʰ* was a later change, though this still occurred fairly early since it is found in Hittite and the other Anatolian daughter languages — this **-tʰ* was added to the 3rd plural ending **-n-* at the same time, yielding the new ending **-ntʰ-*. This **-tʰ* probably had the same origin as the 3rd singular possessive suffix **-t* found in Ugric and some of the Samoyed languages on the one hand and in the Proto-Tungus 3rd plural possessive suffix **-t* on the other (cf. Sinor 1988:727—728). The most recent change must have been the development of the so-called "primary" endings, which were built upon the so-called "secondary" endings by the addition of the deictic particle **-i* meaning "here and now", as shown by Kerns and Schwartz in their 1972 book on Indo-European verb morphology. It may be mentioned that this deictic particle had a Nostratic origin, coming from a widely-represented proximate demonstrative stem meaning 'this one here'.

Now, Proto-Uralic is assumed to have had two conjugational types (cf. Hajdú 1972:43—44; Collinder 1960:308): (A) a determinative (objective) conjugation, which was characterized by the 3rd singular in **-s* and which was used with transitive verbs, and (B) an indeterminative (subjective) conjugation, which was characterized by the 3rd singular in *zero* and which was used with intransitive verbs. The same two conjugational types existed in Proto-Indo-European, except that the contrast was between active and stative. Indeed, the active ~ stative contrast appears to be the more ancient in both Proto-Uralic and Proto-Indo-European.

After all of the changes described above had taken place, the resulting Proto-Indo-European athematic endings were as follows (cf. Brugmann 1904:588—594; Beekes 1995:232—233; Burrow 1973:306—319; Szemerényi 1996:327; Fortson 2004:84—85;

Clackson 2007:123—125; Shields 1992; Meillet 1964:227—232; Watkins 1998:60; Meier-Brügger 2003:178; Adrados 1974.II:619—663; Sihler 1995:454):

	I. Primary		II. Secondary	
Person	Singular	Plural	Singular	Plural
1	*-mi	*-me	*-m	*-me
2	*-si	*-t^{h}e	*-s	*-t^{h}e
3	*-t^{h}i	*-nt^{h}i	*-t^h	*-nt^h

Note: The 1st person plural endings have different extensions in the various daughter languages: _*-me-s(i)_, _*-mo-s(i)_, _*-me-n(i)_, _*-mo-n(i)_. In these endings, the plural markers _*-s_ and _*-n_ have been added to _*-me/*-mo_. It may be noted that the plural marker _*-n_ is also found in Tungus — in Evenki, Even, Solon, Negidal, for example, the 2nd plural possessive suffix is made up of the 2nd singular possessive suffix plus the plural marker _*-n_ (cf. Sinor 1988:727).

In volume 1, Grammar, of his book _Indo-European and Its Closest Relatives: The Eurasiatic Language Family_, Greenberg (2000:67) discusses the evidence for a Eurasiatic first-person singular pronoun stem _*k_. He writes:

> Less widely distributed than _m_ for the first-person singular is _k_. Wherever they both appear, the general contrast is _m_ as ergative versus absolutive _k_, _m_ as active versus middle or passive _k_, and _m_ as active versus stative _k_. I am inclined to believe that this last contrast is the basic one from which the others developed. A contrast of this kind between _m_ and _k_ seems to be attested only in the first-person singular.

Over the past quarter century or so, several scholars have tried to show that Indo-European is to be reconstructed as an active language (for a brief discussion, cf. Schwink 1994:86—87 and 89—110; see also Lehmann 2002). Indeed, such an interpretation seems to clarify many problems in the early dialects. According to this interpretation, the so-called "perfect" of traditional Indo-European is seen as originally stative (cf. Lehmann 1993:218 and 2002:169—172; see Chapters 18 and 19 of my 2008 book for details). Comparison with other Nostratic languages allows us to confirm this view.

The perfect reconstructed by the Neogrammarians for Proto-Indo-European was distinguished from the present and aorist by a unique set of personal endings in the indicative, namely, first person singular _*-ə̯$_2$a_ (cf. Sanskrit _véd-a_ 'I know', Greek οἶδ-α, Gothic _wait_), second person singular _*-tə̯$_2$a_ (cf. Sanskrit _vét-tha_ 'you know', Greek οἶσ-θα, and Gothic _waist_), third person singular _*-e_ (cf. Greek οἶδ-ε 'he/she knows', Sanskrit _véd-a_, and Gothic _wait_). Except for Armenian and Balto-Slavic, the perfect remained in all branches. It was least changed in Indo-Iranian, Celtic, and Germanic. In Greek, however, it was mixed up with a κ-formation and, in Italic, with a whole series of non-perfect tense forms. According to Greenberg, the perfect of traditional comparative grammar was originally stative in Proto-Indo-European, and, as noted above, others have recently made similar assertions. Sihler (1995:564—590) gives an excellent overview of the stative in Indo-European.

Now, Greek has a unique formation, the so-called "κ-perfect". However, this formation arose exclusively within prehistoric Greek. It is already found, to a limited extent, in Homer and in the earliest records of other dialects. In Homer, the formation is found in some 20 roots, all ending in a long vowel, and, in all of them, the κ-stem is virtually limited to the singular stems which actually contain a long vowel. Later, the formation spread to other stems ending in a long vowel, then to stems ending in any vowel (including denominatives), and finally to stems ending in consonants, and to all persons and numbers. Thus, it is clear that we are dealing with developments specific to Greek itself. For a discussion of the Greek perfect, cf. Chantraine 1927.

In Latin, we find first singular perfect forms _fēcī_ 'I did' and _iēcī_ 'I threw'. As in Greek, the _-c-_ [k] is found in all persons (cf. third singular _fecit_), and, as in Greek, the _-c-_ [k] has given rise to secondary formations (such as _faciō_ and _iaciō_, for example).

The _-k-_ forms are also found in Tocharian, as in first singular preterite active _tākā_ 'I was', and, as in Greek and Latin, the _-k-_ is found in all persons and has given rise to secondary formations. Van Windekens (1976—1982.I:495—496) goes so far as to posit Proto-Indo-European *_dhēq-_, *_dhə_$_1$_q-_ as the source of Tocharian _tākā_ 'I was'.

On the basis of the evidence from Greek, Latin, and Tocharian, we may assume that a "suffix" *_-k-_ is to be reconstructed for late-stage Proto-Indo-European, that is, what I refer to as "Disintegrating Indo-European". This "suffix" originally had a very limited distribution — it seems to have appeared only in the perfect singular of verbs that ended in a long vowel, when the long vowel originated from earlier short vowel plus laryngeal. All of the other formations found in Greek, Italic, and Tocharian are secondary elaborations. But, we can go back even farther — we can speculate that the _-k-_ originally characterized the first person exclusively, from which it spread to other persons. This suggestion is not new. Sturtevant (1942:87—88) suggested that *_-k-_ developed in the first person singular when a root-final laryngeal was followed by the ending *_-xe_ (that is, *_-H_$_2$_e_ [Kuryłowicz would write *_-ə̯_$_2$_e_]). Though a laryngeal explanation along these lines has not been generally accepted (cf. Messing 1947:202—203), the suggestion that the _-k-_ was originally confined to the first person singular is still a viable hypothesis, especially in view of the evidence from other Nostratic languages. Thus, both in function and form, the first singular *_-k-_ ending would belong with the Eurasiatic first person singular pronoun stem *_k_ reconstructed by Greenberg. It should be noted that this explanation is different than that given by Greenberg, who compares the Proto-Indo-European first person perfect (stative) ending *_-Ha_ with the *_-k-_ endings found in the other Eurasiatic languages. On purely phonological grounds, I find Greenberg's proposal less convincing than the alternative suggested here. Moreover, the first person perfect ending *_-Ha_ has a exact match in

Elamite (see above), which clearly shows that it was inherited from Proto-Nostratic and, thus, not related to the *-*k*- endings under discussion here.

12. Prohibitive/Negative Particles and Indeclinables

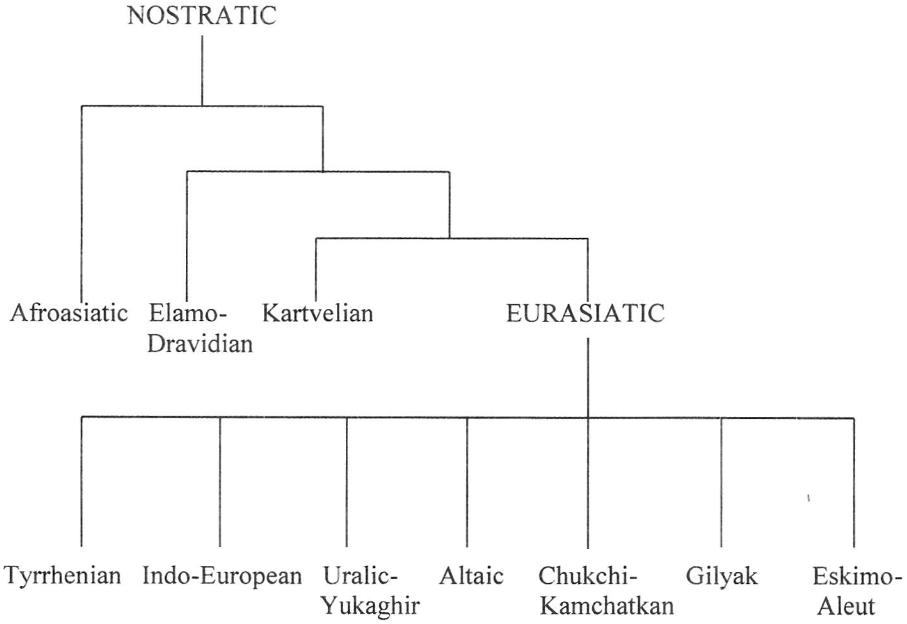
The following negative/prohibitive particles and indeclinables can be reconstructed for Proto-Nostratic:

Negative particles: **na*, **ni*, **nu*
Prohibitive particle: **ma(ʔ)*
Negative particle: **ʔal-* (~ **ʔəl-*)
Negative particle: **li* (~ **le*) (?)
Negative particle: **ʔe*
Post-positional intensifying and conjoining particle: **kʷʰa-* (~ **kʷʰə-*)
Particle: **kʷʰay-* 'when, as, though, also'
Particle: **ħarʸ-* 'or; with, and; then, therefore'
Particle: **ʔin-* (~ **ʔen-*), **(-)ni* 'in, into, towards, besides, moreover'
Sentence particle: **wa* (~ **wə*) 'and, also, but; like, as'
Coordinating conjunction: **ʔaw-*, **ʔwa-* (~ **ʔwə-*) 'or'

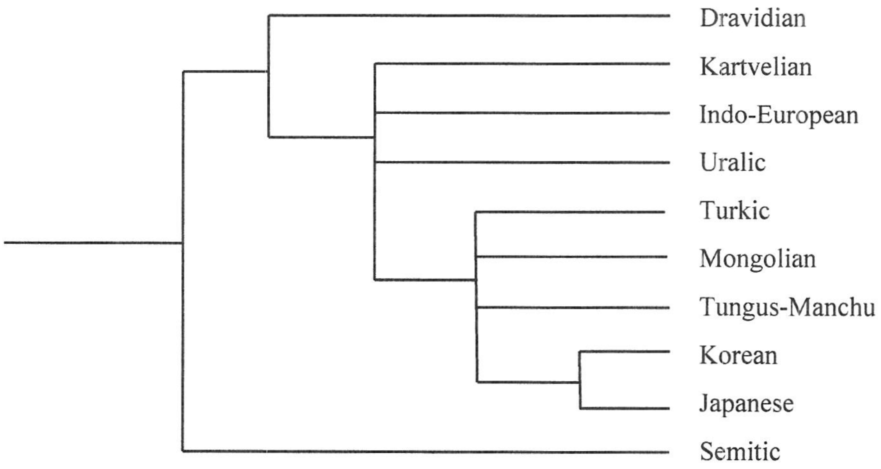
Note: The *CVC-* root structure patterning of some of these forms points to their ultimate nominal or verbal origin. For example, the negative particle **ʔal-* (~ **ʔəl-*) must ultimately have been a negative verb stem meaning 'to be not so-and-so', as in its Dravidian derivatives, while **ʔin-* (~ **ʔen-*), **(-)ni* was originally a nominal stem meaning 'place, location' (cf. Dolgopolsky to appear, no. 45, **ʔin[A]* 'place' [(in descendant languages) → 'in']).

Appendix 1: The Nostratic Macrofamily

The following chart represents the subgrouping of the Nostratic daughter languages. Somewhat similar views are expressed by Sergej Starostin (1999:66) in a computer-generated Nostratic family tree, though he places Kartvelian closer to Indo-European than what is indicated below, and he lists Semitic as a separate branch of Nostratic — clearly, this should be Afroasiatic.


NOSTRATIC
Afroasiatic
Elamo-Dravidian
Kartvelian
EURASIATIC
Tyrrhenian
Indo-European
Uralic-Yukaghir
Altaic
Chukchi-Kamchatkan
Gilyak
Eskimo-Aleut

Sergej Starostin's computer-generated genealogical tree for Nostratic is as follows:


Dravidian
Kartvelian
Indo-European
Uralic
Turkic
Mongolian
Tungus-Manchu
Korean
Japanese
Semitic

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

Appendix 2:
From Proto-Nostratic to Proto-Afroasiatic: Preliminary Thoughts

Though significant progress has been made in reconstructing the Proto-Afroasiatic phonological system and vocabulary, Proto-Afroasiatic morphology has not yet been reconstructed. Nevertheless, it is possible to trace, in broad outline, some of the developments that may have occurred, though much still remains uncertain.

Though Afroasiatic plays a critical role in the reconstruction of Proto-Nostratic morphology, there were many developments that occurred within Proto-Afroasiatic proper after it became separated from the rest of the Nostratic speech community. In this appendix, an attempt will be made to provide explanations for how some of the unique characteristics of Proto-Afroasiatic morphology may have come into being.

1. GENDER: Proto-Nostratic nouns did not distinguish gender, and Pre-Proto-Afroasiatic nouns must also have lacked this category. However, based upon the evidence of the Afroasiatic daughter languages, gender must be reconstructed as an inherent part of noun morphology in Proto-Afroasiatic proper.

   Like Proto-Nostratic, Proto-Afroasiatic was most likely an active language. Two declensional types were inherited by Proto-Afroasiatic from Proto-Nostratic, each of which was distinguished by a special set of markers:

   1. *-*i*/*u* was used to mark the subject in active constructions;
   2. *-*a* was used to mark:

      (a) The direct object of transitive verbs;
      (b) The subject in stative constructions;
      (c) The so-called "*status indeterminatus*".

   Now, Sasse (1984:117) reconstructs the following two declensional paradigms for nouns with short final vowels for Proto-East Cushitic:

	Masculine	Feminine
Absolute Case	*-a	*-a
Subject Case	*-u/i	*-a

   Note: The absolute case is not to be confused with the "absolutive" case of ergative languages. It is a translation of Italian *forma assoluta* first used by Moreno in 1935 (cf. Mous 2012:369).

Sasse notes:

> Regardless of whether the neutralization of the case forms in the feminine nouns was inherited from the proto-language (that is, case forms for feminines never developed) or represents a historical stage during the reduction of the case-marking system which was once more elaborate, it is obvious that the lack of subject-object distinction with feminine nouns can be explained in functional terms. It is well known that in addition to the semantic category of neutral sex which is of minor importance the Cushitic gender categories primarily denote the notions of social significance (masculine) vs. social insignificance (feminine)... Since the primary function of subject and object cases is the distinction of agent and patient nouns, it is clear that case marking is more important for those noun classes that are designated to denote items which normally occur on both agents and patients (i.e. animates, big and strong beings, etc.) than for those noun classes which do not (inanimates, insignificant things, etc.). There is an interesting parallel in Indo-European, where neuter nouns generally do not distinguish subject and object. The personal pronouns and the demonstratives are naturally excluded from this neutralization, because they are more likely to refer to animates.

Thus, the feminine forms reconstructed for Proto-East Cushitic by Sasse are to be derived from the **-a* found in the masculine absolute. This must have been the oldest patterning, and, inasmuch as there are traces of this patterning in Berber and Semitic, it must ultimately go back to Proto-Afroasiatic. Once the category of gender was firmly established in Afroasiatic, the individual daughter languages exploited other means to indicate the feminine, such as, for example, the formant **-t-*. For more information on how the category of gender is treated in the various branches, cf. especially D. Cohen (ed.) 1988 and Fajzyngier—Shay (eds.) 2012.

2. PRONOUNS: Proto-Afroasiatic had independent personal pronouns distinct from subject and object pronouns. The following independent personal pronouns may be reconstructed for Pre-Proto-Afroasiatic:

	Singular	Plural
1	*ʔV-	*nV+Plural
2	*tV-	*tV+Plural
3	*sV-	*sV+Plural

Notes:

1. The first and second person forms were exactly as given above for the prefix conjugation personal prefixes, except that the third person prefix was based upon the stem **yV-*. This is an important piece of information, for it allows us to ascertain what the most archaic forms of the personal pronouns may have been and to speculate about their later development.

2. In Omotic, the first person is built upon the stem **ta-* and the second upon the stem **ne-* (cf. Welaitta 1st sg. subject *ta-ni*, 2nd sg. subject *ne-ni*).

It should be noted that the first person singular and plural were originally two distinct stems. The first innovation was the combining of the two first person stems into a new compound form:

	Singular	Plural
1	*ʔV+nV-	*ʔV+nV+Plural
2	*tV-	*tV+Plural
3	*sV-	*sV+Plural

Then, **ʔV-* was extended to the second and third person forms in imitation of the first person forms:

	Singular	Plural
1	*ʔV+nV-	*ʔV+nV+Plural
2	*ʔV+tV-	*ʔV+tV+Plural
3	*ʔV+sV-	*ʔV+sV+Plural

Next, **-n-* was angalogically inserted into the second person forms on the basis of the first person forms:

	Singular	Plural
1	*ʔV+nV-	*ʔV+nV+Plural
2	*ʔV+n+tV-	*ʔV+n+tV+Plural
3	*ʔV+sV-	*ʔV+sV+Plural

Finally, separate feminine third person forms were created.

No doubt, the changes described above occurred over a long period of time and may not have been fully completed by the time that the individual Afroasiatic daughter languages began to appear. Each daughter language, in turn, modified the inherited system in various ways. Here are attested forms in select Afroasiatic daughter languages (only the singular and plural forms are given) (cf. Lipiński 1997:298—299; Moscati 1964:102; R. Stempel 1999:82; Diakonoff 1988:72—73; Gray 1934:62; Gardiner 1957:53; Frajzyngier—Shay [eds.] 2012):

	Semitic: Arabic	Egyptian	Berber: Tuareg	Cushitic: Rendille
Singular				
1	ˀanā	ỉn-k	n-ək	an(i)
2 (m.)	ˀanta	nt-k	kay	at(i)
(f.)	anti	nt-ṯ	kəm	at(i)
3 (m.)	huwa	nt-f	nt-a	us(u)
(f.)	hiya	nt-s	nt-a	iče
Plural				
1 (m.)	naḥnu	ỉn-n	n-əkkă-ni	inno
(f.)	naḥnu	ỉn-n	n-əkkă-nəti	inno
2 (m.)	ˀantum(ū)	nt-ṯn	kăw-ni	atin
(f.)	ˀantunna	nt-ṯn	kămă-ti	atin
3 (m.)	hum(ū)	nt-sn	əntă-ni	ičo
(f.)	hunna	nt-sn	əntă-nəti	ičo

3. CONJUGATION: Proto-Afroasiatic had two conjugations: (1) a prefix conjugation (active) and (2) a suffix conjugation (stative). The prefix conjugation became fixed in Proto-Afroasiatic, while the suffix conjugation was flexible. Thus, the various daughter languages inherited a common prefix conjugation from Proto-Afroasiatic (except for Egyptian), while the suffix conjugations differed slightly from branch to branch. The Proto-Afroasiatic personal prefixes were as follows (cf. Diakonoff 1988:80; D. Cohen 1968:1309; Lipiński 1997:370—371):

	Singular	Plural
1	*ʔV-	*nV-
2	*tV-	*tV-
3 (m.)	*yV-	*yV-
(f.)	*t-	*yV-

It is instantly obvious that these prefixes are based upon earlier Proto-Nostratic pronominal elements. It should be noted here that Banti (2004:40) reconstructs a nearly identical set of forms for the Proto-Cushitic *suffix* conjugation (SC1):

	Singular	Plural
1	*Stem-*ʔV*	*Stem-*anV* (?)
2	*Stem-*tV*	*Stem-*tin*
3 (m.)	*Stem-*i*	*Stem-*in*
(f.)	*Stem-*tV*	

Notes:

1. The 2nd and 3rd plural forms contain the plural marker *-*n* (cf. Bomhard 2008.I: 311—314, §16.26; Dolgopolsky to appear, no. 1522). Similar forms are found in several Indo-European daughter languages (for example, Hittite and Greek).
2. Masculine and feminine are not distinguished in the 3rd plural.

4. ORIGIN OF APOPHONY: The Proto-Afroasiatic root structure patterning may be reconstructed as follows:

    1. There were no initial vowels in the earliest form of Proto-Afroasiatic. Therefore, every root began with a consonant.
    2. Originally, there were no initial consonant clusters either. Consequently, every root began with one and only one consonant.
    3. Two basic syllable types existed: (A) **CV* and (B) **CVC*, where *C* = any non-syllabic and *V* = any vowel. Permissible root forms coincided with these two syllable types.
    4. A verb stem could either be identical with a root or it could consist of a root plus a single derivational morpheme added as a suffix to the root: **CVC-(V)C-*. Any consonant could serve as a suffix.
    5. Primary (that is, non-derivational) noun stems displayed similar patterning, though, unlike verb stems, they were originally characterized by stable vocalism.

One of the most striking characteristics of the Semitic verb is the overwhelming preponderance of triconsonantal roots: CCC. Another salient characteristic is that the lexical meaning falls exclusively on the consonants. The vowels, on the other hand, alternate according to well-defined patterns that indicate specific inflectional and derivational functions. That is to say, the vowels have morphological rather than semantic significance. This alternation of vowels is technically known as "apophony". The triconsonantal template and the apophonic alternations form a tightly integrated system.

Earlier in this paper, it was suggested that the formative vowels may have been aspect markers. According to Zaborski, the patterning was as follows: *a* marks present (imperfective), *i* ~ *e* mark past (perfective), and *u* ~ *o* mark subordinate. Thus, following Zaborski's views, the Proto-Afroasiatic active verb stems would have had the following patterning:

Imperfective aspect	*CVCaC-
Perfective aspect	*CVCiC-
Subordinate	*CVCuC-

At this stage, the vowel of the first syllable was stable, while that of the second syllable changed as indicated above.

The innovation that led to the rise of apophony was the modification of the vowel of the first syllable to indicate different morphological functions in imitation of the patterning of the second syllable. A repercussion of the rise of apophony was the need to bring all verbal roots into conformity with the triconsonantal scheme, at the expense of other root types. The reason for this was that the emerging apophonic patterning could only function properly within the context of a fairly rigid structure. This system became so tightly integrated that it was, for all practical purposes, impervious to further change. Even to the present day, the verbal patterning is highly homologous among the Semitic daughter languages. For details, see especially Diakonoff 1988:85—110 and Kuryłowicz 1962. Rössler 1981 is also of interest.

5. STATE: Proto-Semitic nouns had two distinct forms, depending upon their syntactic function: (1) construct state; (2) free state (additional states developed in the daughter languages). The construct state was used when a noun governed a following element. It had no special marker and was the unmarked form. The free state was used elsewhere and was the marked form. It was indicated by the markers **-m(a)*/**-n(a)*, which were appended after the case endings (cf. Rubin 2010:38—40). Ultimately, these markers had the same origin as the relational markers **-ma* and **-na*, which were originally used to mark the direct object of transitive verbs as well as the subject in stative constructions (see above, §7; see also Michalove 1992:94, note 2). In Proto-Semitic, they were reinterpreted as markers of the free state.

References

Abondolo, Daniel
1998 "Introduction", in: Daniel Abondolo (ed.), *The Uralic Languages*. London and New York, NY: Routledge, pp. 1—42.
Abondolo, Daniel (ed.)
1998 *The Uralic Languages*. London and New York, NY: Routledge.

Adams, Douglas Q.
1988 *Tocharian Historical Phonology and Morphology*. New Haven, CT: American Oriental Society.

Adrados, Francisco R[odríguez]
1975 *Lingüística indoeuropea [Indo-European Linguistics]*. 2 vols. Madrid: Gredos.

Adrados, Francisco R., Alberto Bernabé, and Julia Mendoza
1995—1998 *Manual de lingüística indoeuropea [Manual of Indo-European Linguistics]*. 3 vols. Madrid: Ediciones Clásicas.
[2010] [English translation. Vol. I. Louvain: Peeters.]

Appleyard, D[avid] L.
2007a "Beja Morphology", in: Alan S. Kaye (ed.), *Morphologies of Asia and Africa*. Winona Lake, IN: Eisenbrauns, Vol. 1, pp. 447—479.
2007b "Bilin Morphology", in: Alan S. Kaye (ed.), *Morphologies of Asia and Africa*. Winona Lake, IN: Eisenbrauns, Vol. 1, pp. 481—504.

Austerlitz, Robert
1968 "L'ouralien" [Uralic], in: André Martinet (ed.), *Le langage [Language]*. Paris: Éditions Gallimard, pp. 1331—1387.

Banti, Giorgio
2004 "New Perspectives on the Cushitic Verbal System", in: Andrew Simpson (ed.), *Proceedings of the Twenty-Seventh Annual Meeting of the Berkeley Linguistics Society (March 22—25, 2001) — Special Session on Afroasiatic Languages*. Berkeley, CA: Berkeley Linguistics Society, pp. 1—48.

Beekes, Robert S. P.
1995 *Comparative Indo-European Linguistics: An Introduction*. Amster-dam and Philadelphia, PA: John Benjamins.
[2011] [2nd edition, revised and corrected by Michiel de Vaan.]

Bender, M. Lionel
2000 *Comparative Morphology of the Omotic Languages*. (= *LINCOM Studies in African Linguistics 19*.) München: Lincom Europa.
2007 "Topics in Omotic Morphology", in: Alan S. Kaye (ed.), *Morphologies of Asia and Africa*. Winona Lake, IN: Eisenbrauns, Vol. 1, pp. 729—751.

Benveniste, Émile
1935 *Origines de la formation des noms en indo-européen [Origins of the Formation of Nouns in Indo-European]*. Paris: Adrien-Maisonneuve.
1948 *Noms d'agent et noms d'action en indo-européen [Agent Nouns and Action Nouns in Indo-European]*. Paris: Adrien-Maisonneuve.

Bomhard, Allan R.
2004 "Preliminary Thoughts on Nostratic Morphology", in: Irén Hegedűs and Paul Sidwell (eds.), *Nostratic Centennial Conference: The Pécs Papers*. Pécs, Hungary: Lingua Franca Group, pp. 27—50.
2008 *Reconstructing Proto-Nostratic: Comparative Phonology, Morphology, and Vocabulary*. 2 volumes. Leiden and Boston, MA: E. J. Brill. (Second revised, corrected, and expanded edition in preparation.)
2011 *The Nostratic Hypothesis in 2011*. Washington, DC: Institute for the Study of Man.

Bomhard, Allan R. and John C. Kerns
1994 *The Nostratic Macrofamily: A Study in Distant Linguistic Relation-ship*. Berlin, New York, NY, and Amsterdam: Mouton de Gruyter.

Brugmann, Karl
1888—1895 *Elements of the Comparative Grammar of the Indo-Germanic Languages*. English translation by Joseph Wright, R. Seymour Conway, and W. H. D. Rouse. 5 vols. New York, NY: B. Westermann and Co.
1904 *Kurze vergleichende Grammatik der indogermanischen Sprachen. Auf Grund des fünfbändigen "Grundriß der vergleichenden Grammatik der indogermanischen Sprachen von K. Brugmann und B. Delbrück" [Concise Comparative Grammar of the Indo-European Languages. Based upon the Five Volume "Elements of the Comparative Grammar of the Indo-European Languages by K. Brugmann and B. Delbrück"].* Berlin: Walter de Gruyter. Reprinted 1970.

Brugmann, Karl and Berthold Delbrück
1897—1916 *Grundriß der vergleichenden Grammatik der indogermanischen Sprachen [Elements of the Comparative Grammar of the Indo-European Languages].* 2nd edition. Berlin: Walter de Gruyter. Reprinted 1967.

Buck, Carl Darling
1933 *Comparative Grammar of Greek and Latin*. 10th impression 1966. Chicago, IL, and London: University of Chicago Press.

Burrow, Thomas
1973 *The Sanskrit Language.* 3rd edition. London: Faber & Faber.

Bush, Frederic William
1964 *A Grammar of the Hurrian Language*. Ph.D. Dissertation, Brandeis University.

Caldwell, Robert

1913 *A Comparative Grammar of the Dravidian or South Indian Family of Languages.* 3rd edition revised and edited by J. L. Wyatt and T.

Ramakrishna Pillai. Reprinted 1974. New Delhi: Oriental Reprint Corporation.

Cavoto, Fabrice
1998 *Histoire du fennique et de l'ouralien dans la perspective des recherches nostratiques [History of Fennic and Uralic from the Perspective of Nostratic Research]*. Manuscript.

Chantraine, Pierre
1927 *Histoire du parfait grec [History of the Greek Perfect]*. Paris: Honoré Champion.

Clackson, James P. T.
2007 *Indo-European Linguistics: An Introduction*. Cambridge: Cambridge University Press.

Cohen, David (ed.)
1988 *Les langues dans le monde ancien et moderne: langues chamito-sémitiques [Languages in the Ancient and Modern World: Hamito-Semitic Languages]*. Paris: Centre National de la Recherche Scientifique.

Collinder, Björn
1934 *Indo-uralisches Sprachgut: Die Urverwandtschaft zwischen der indoeuropäischen und der uralischen (finnisch-ugrisch-samojedischen) Sprachfamilie [Common Indo-Uralic Linguistic Possessions: The Primitive Relationship between the Indo-European and Uralic (Finno-Ugrian-Samoyed) Language Families]*. (= *Uppsala Universitets Årsskrift 1934, Filosofi, Språkvetenskap och Historiska Vetenskaper 1*.) Uppsala: A.-B. Lundequistska Bokhandeln, pp. 1—116.
1960 *A Comparative Grammar of the Uralic Languages*. Uppsala: Almqvist and Wiksells.
1965 *An Introduction to the Uralic Languages*Berkeley and Los Angeles, CA: University of California Press.

Comrie, Bernard
1976 *Aspect*. Reprinted 1978. Cambridge: Cambridge University Press.

Crystal, David
1992 *An Encyclopedic Dictionary of Language and Languages*. Oxford: Blackwell.

Décsy, Gyula
1965 *Einführung in die finnisch-ugrische Sprachwissenschaft [Introduction to Finno-Ugrian Linguistics]*. Wiesbaden: Otto Harrassowitz.
1990 *The Uralic Protolanguage: A Comprehensive Reconstruction*. Bloomington, IN: Eurolingua.

1991 *The Indo-European Protolanguage: A Computational Reconstruction*. Bloomington, IN: Eurolingua.
1998 *The Turkic Protolanguage: A Computational Reconstruction*. Bloomington, IN: Eurolingua.
2002 *The Afroasiatic Protolanguage. An Attempt at a Combined Phylogenetic and Historical-Comparative Reconstruction with Anthropological Objectives*. Bloomington, IN: Eurolingua.

Diakonoff, Igor M.
1965 *Semito-Hamitic Languages*. Moscow: Nauka.
1975 "On Root Structure in Proto-Semitic", in: James Bynon and Theodora Bynon (eds.), *Hamito-Semitica*. The Hague: Mouton, pp. 133—153.
1988 *Afrasian Languages*. Moscow: Nauka.

Diakonoff, I. M., A. Yu. Militarëv, V. Ya. Porxomovsky, and O. V. Stolbova
1987 "Общеафразийская фонологическая система [Common Afrasian Phonological System], in: V. Ya. Porxomovsky (ed.), *Африканское историческое языкознание: проблемы реконструкции [African Historical Linguistics: Problems of Reconstruction]*. Moscow: Nauka, pp. 9—29.

Dixon, R. M. W.
1979 "Ergativity", *Language* 55:59—138.
1994 *Ergativity*. Cambridge: Cambridge University Press.

Dolgopolsky, Aaron
1984 "On Personal Pronouns in the Nostratic Languages", in: Otto Gschwantler, Károly Rédei, and Hermann Reichert (eds.), *Linguistica et Philologica. Gedenkschrift für Björn Collinder (1894—1983) [Linguistics and Philology. Memorial Volume for Björn Collinder (1894—1983)]*. Vienna: Wilhelm Braumüller, pp. 65—112.
1994 "Nostratic", in: R. E. Asher (ed.), *The Encyclopedia of Language and Linguistics*. Oxford: Pergamon Press, vol. 5, p. 2838.
1998 *The Nostratic Hypothesis and Linguistic Paleontology*. Cambridge: The McDonald Institute for Archaeological Research.
2005 "Nostratic Grammar: Synthetic or Analytic?", Orientalia et Classica. Труды Института Восточных культур и античности, вып. VI: Аспекты компаративистики 1. М., РГГУ, 2005. С. 13—38.
To appear *Nostratic Dictionary*. Cambridge: The McDonald Institute for Achaeological Research. A draft version was published on-line in 2008 at: http://www.dspace.cam.ac.uk/handle/1810/196512; this is the version cited here.

Dybo, Vladimir
2004 "On Illič-Svityč's Study 'Basic Features of the Proto-Language of the Nostratic Language Family'," in: Irén Hegedűs and Paul Sidwell (eds.), *Nostratic Centennial Conference: The Pécs Papers*. Pécs: Lingua Franca Group, pp. 115—119.

Ehret, Christopher
1980 *The Historical Reconstruction of Southern Cushitic Phonology and Vocabulary*. Berlin: Dietrich Reimer Verlag.
1989 "The Origin of Third Consonants in Semitic Roots: An Internal Reconstruction (Applied to Arabic)", *Journal of Afroasiatic Languages* 2.2:109—202.
1991 "Revising the Consonant Inventory of Proto-Eastern Cushitic", *Studies in African Linguistics* 22.3:211—275.
1995 *Reconstructing Proto-Afroasiatic (Proto-Afrasian): Vowels, Tone, Consonants, and Vocabulary*. Berkeley and Los Angeles, CA: University of California Press.
2003a "The Third Consonants in Ancient Egyptian", in: Gábor Takács (ed.), *Egyptian and Semito-Hamitic (Afroasiatic) Studies in Memoriam Werner Vycichl*. Leiden and Boston, MA: E. J. Brill, pp. 33—54.
2003b "Third Consonants in Chadic Verbal Roots", in: M. Lionel Bender, Gábor Takács, and David Appleyard (eds.), *Selected Comparative-Historical Afrasian Linguistic Studies in Memory of Igor Diakonoff*. Munich: LINCOM Europa, pp. 61—69.
2008 "The Internal and Comparative Reconstruction of Verb Extensions in Early Chadic and Afroasiatic", in Zygmunt Frajzyngier and Erin Shay (eds.), *Interaction of Morphology and Syntax: Case Studies in Afroasiatic*. Amsterdam and Philadelphia, PA: John Benjamins, pp. 41—59.

Fähnrich, Heinz
1993 *Kurze Grammatik der georgischen Sprache [Short Grammar of the Georgian Language]*. Third Edition. Leipzig, Berlin, München: Langenscheidt.
1994 *Grammatik der altgeorgischen Sprache [Grammar of the Old Georgian Language]*. Hamburg: Helmut Buske Verlag.
2007 *Kartwelisches etymologisches Wörterbuch [Kartvelian Etymological Dictionary]*. Leiden: E. J. Brill.

Fähnrich, Heinz and Surab Sardshweladse
1995 *Etymologisches Wörterbuch der Kartwel-Sprachen [Etymological Dictionary of the Kartvelian Languages]*. Leiden: E. J. Brill.

Fortescue, Michael
1998 *Language Relations across Bering Strait: Reappraising the Archaeological and Linguistic Evidence*. London: Cassell.
2005 *Comparative Chukotko-Kamchatkan Dictionary*. Berlin: Mouton de Gruyter.
2011 "The Relationship of Nivkh to Chukoto-Kamchatkan Revisited", *Lingua* 121:1359—1376.
Fortson, Benjamin W., IV
2004 *Indo-European Language and Culture: An Introduction*. Oxford: Blackwell.
[2010] [2nd edition. Chichester: Wiley-Blackwell.]
Frajzyngier, Zygmunt, and Erin Shay (eds.)
2012 *The Afroasiatic Languages*. Cambridge: Cambridge University Press.
Friedrich, Johannes
1969a "Churritisch" [Hurrian], in: B. Spuler (ed.), *Altkleinasiatische Sprachen [Ancient Near Eastern Languages]*. Leiden: E. J. Brill, pp. 1—30.
1969b "Urartäisch" [Urartian], in: B. Spuler (ed.), *Altkleinasiatische Sprachen [Ancient Near Eastern Languages]*. Leiden: E. J. Brill, pp. 31—53.
Gamkrelidze, Thomas V. and Vjačeslav V. Ivanov
1995 *Indo-European and the Indo-Europeans: A Reconstruction and Historical Typological Analysis of a Protolanguage and a Proto-Culture.* 2 vols. English translation by Johanna Nichols. Berlin, New York, NY, and Amsterdam: Mouton de Gruyter.
Gamkrelidze, Thomas V. and Givi Mačavariani
1982 *Sonantsystem und Ablaut in den Kartwelsprachen. Eine Typologie der Struktur des Gemeinkartwelischen [The System of Resonants and Ablaut in the Kartvelian Languages. A Typology of the Structure of Common Kartvelian]*. German translation by Winfried Boeder. Tübingen: Gunter Narr Verlag.
Gardiner, Alan
1957 *Egyptian Grammar*. 3rd edition. Oxford: Oxford University Press.
Gray, Louis H.
1934 *An Introduction to Semitic Comparative Linguistics*. Reprinted 1971. Amsterdam: Philo Press.
Greenberg, Joseph H.
1990 "The Prehistory of the Indo-European Vowel System in Comparative and Typological Perspective", in: Vitaly Shevoroshkin (ed.), *Proto-Languages and Proto-Cultures*. Bochum: Brockmeyer, pp. 77—136.
2000—2002 *Indo-European and Its Closest Relatives: The Eurasiatic Language Family*. 2 vols. Stanford, CA: Stanford University Press.

Grillot-Susini, Françoise
1987 *Éléments de grammaire élamite [Elements of Elamite Grammar]*. Paris: Éditions Recherche sur les Civilizations.

Grønbech, Kaare and John R. Krueger
1993 *An Introduction to Classical (Literary) Mongolian*. Third (corrected) edition. Wiesbaden: Otto Harrassowitz.

Gruzdeva, Ekaterina
1998 *Nivkh*. München: Lincom Europa.

Hajdú, Péter
1972 "The Origins of Hungarian", in: Loránd Benkő and Samu Imre (eds.), *The Hungarian Language*. The Hague: Mouton, pp. 15—48.
1975 *Finno-Ugrian Languages and Peoples*. Translated and adapted by G. F. Cushing. London: Andre Deutsch.

Haudry, Jean
1979 *L'indo-européen [Indo-European]*. Paris: Presses Universitaires de France.

Hegedűs, Irén
1992 "Reconstructing Nostratic Morphology: Derivational Elements", in: Vitaly Shevoroshkin (ed.), *Nostratic, Dene-Caucasian, Austric and Amerind*. Bochum: Brockmeyer, pp. 34—47.
1997 "On Grammaticalization in Nostratic", in: Irén Hegedűs, Peter A. Michalove, and Alexis Manaster Ramer (eds.), *Indo-European, Nostratic, and Beyond: Festschrift for Vitalij V. Shevoroshkin*. Washington, DC: Institute for the Study of Man, pp. 106—115.

Hetzron, Robert (ed.)
1997 *The Semitic Languages*. London and New York, NY: Routledge.

Hudson, Grover
1976 "Highland East Cushitic", in: M. Lionel Bender (ed.), *The Non-Semitic Languages of Ethiopia*. East Lansing, MI: African Studies Center, Michigan State University, pp. 232—277.

Illič-Svityč, V[ladislav] M[arkovič]
1965 "Материалы к сравнительному словарю ностратических языков (индоевропейский, алтайский, уральский, дравидский, картвельский, семитохамитский)" [Materials for a Comparative Dictionary of the Nostratic Languages (Indo-European, Altaic, Uralic, Dravidian, Kartvelian, Hamito-Semitic)], *Этимология* (*Ėtimologija*) 1965:321—373.
1971— 1984 *Опыт сравнения ностратических языков (семито-хамитский, картвельский, индоевропейский, уральский, дравидийский,*

алтайский) [An Attempt at a Comparison of the Nostratic Languages (Hamito-Semitic, Kartvelian, Indo-European, Uralic, Dravidian, Altaic)]. 3 vols. Moscow: Nauka.

Johanson, Lars and Éva Ágnes Csató (eds.)
1998 *The Turkic Languages*. London and New York, NY: Routledge.

Kerns, J. Alexander, and Benjamin Schwartz
1972 *A Sketch of the Indo-European Finite Verb*. Leiden: E. J. Brill.

Kerns, John C.
1985 *Indo-European Prehistory*. Cambridge: Heffer and Sons.

Khačikjan, Margaret
1998 *The Elamite Language*. Rome: Consiglio Nazionale delle Ricerche, Istituto per gli Studi Micenei ed Egeo-anatolici.

Klimov, G[eorgij] V.
1964 *Этимологический словарь картвельских языков [Etymological Dictionary of the Kartvelian Languages]*. Moscow: Nauka.
1969 *Die kaukasischen Sprachen [The Caucasian Languages]*. German translation by Winfried Boeder. Hamburg: Helmut Buske Verlag.
1977 *Типология языков активного стоя [The Typology of Languages of Active Formation]*. Moscow: Nauka.
1994 *Древнейшие индоевропеизмы картвельских языков [Ancient Indo-European and Kartvelian Languages]*. Moscow: Publications of the Linguistic Institute of the Russian Academy of Sciences.
1998 *Etymological Dictionary of the Kartvelian Languages*. Berlin and New York, NY: Mouton de Gruyter.

Kortlandt, Frederik
2010a "C. C. Uhlenbeck on Indo-European, Uralic, and Caucasian", in: Frederik Kortlandt, *Studies in Germanic, Indo-European, and Indo-Uralic*. Amsterdam and New York, NY: Rodopi, pp. 31—36.
2010b "Indo-Uralic and Altaic", in: Frederik Kortlandt, *Studies in Germanic, Indo-European, and Indo-Uralic*. Amsterdam and New York, NY: Rodopi, pp. 415—418.
2010c "Indo-Uralic and Altaic Revisited", in: Frederik Kortlandt, *Studies in Germanic, Indo-European, and Indo-Uralic*. Amsterdam and New York, NY: Rodopi, pp. 419—428.

Kossmann, Maarten G.
2012 "Berber", in: Zygmunt Frajzyngier and Erin Shay (eds.), *The Afroasiatic Languages*. Cambridge: Cambridge University Press, pp. 18—101.

Krishnamurti, Bhadriraju
2003 *The Dravidian Languages*. Cambridge: Cambridge University Press.

Kroeber, Alfred L.
1911 "Yuki", in: A[lfred] L. Kroeber, *The Languages of the Coast of California North of San Francisco* (= University of California Publications in American Archaeology and Ethnology, vol. 9, no. 3, pp. 273—435). Berkeley, CA: University of California Press, pp. 348—381.

Kuryłowicz, Jerzy
1962 *L'apophonie en sémitique [Apophony in Semitic].* Wrocław: Polish Academy.

Laroche, Emmanuel
1980 *Glossaire de la langue hourrite [Glossary of the Hurrian Language].* Paris: Éditions Klincksieck.

Lehmann, Winfred P.
1993 *Theoretical Bases of Indo-European Linguistics.* London, Boston, MA, Melbourne, and Henley: Routledge.
1995 *Residues of Pre-Indo-European Active Structure and their Implications for the Relationships among the Dialects*. Innsbruck: Innsbrucker Beiträge zur Sprachwissenschaft.
1999 "Towards a History of Early Indo-European", *Diachronica* XVI.1:67—95.
2002 *Pre-Indo-European.* (= *Journal of Indo-European Studies Monograph no. 41*.) Washington, DC: Institute for the Study of Man.
2004 "Derivational Morphology of the Early Indo-European Verb", in: Philip Baldi and Pietro U. Dini (eds.), *Studies in Baltic and Indo-European Linguistics in Honor of William R. Schmalstieg*. Amsterdam and Philadelphia, PA: John Benjamins, pp. 113—124.

Lipiński, Edward
1997 *Semitic Languages: Outline of a Comparative Grammar*. Leuven: Peeters.

Marcantonio, Angela
2002 *The Uralic Language Family: Facts, Myths and Statistics*. Oxford and Boston, MA: The Philological Society.

McAlpin, David W.
1974a "Toward Proto-Elamo-Dravidian", *Language* 50:89—101.
1974b "Elamite and Dravidian: The Morphological Evidence", *International Journal of Dravidian Linguistics* 3:343—358.
1975 "Elamite and Dravidian: Further Evidence of the Relationship", *Current Anthropology* 16:105—115.
1981 *Proto-Elamo-Dravidian: The Evidence and Its Implications.* Philadelphia, PA: American Philosophical Society.

Meier-Brügger, Michael
2003 *Indo-European Linguistics*. Berlin: Walter de Gruyter.

Meillet, Antoine
1964 *Introduction à l'étude comparative des langues indo-européennes [Introduction to the Comparative Study of the Indo-European Languages].* University, AL: University of Alabama Press. Reprint of 8th edition (1937).

Menges, Karl H.
1968 *The Turkic Languages and Peoples*. Wiesbaden: Otto Harrassowitz.

Messing, Gordon M.
1947 "Selected Studies in Indo-European Phonology", *Harvard Studies in Classical Philology* 56/57:161—232.

Michalove, Peter A.
2002 "The Nostratic 'Accusative' in **-mA*: An Altaic Perspective", *Anthropological Linguistics* 44/1:85—95.

Mithun, Marianne
1991 "Active/Agent Case Marking and Its Motivations", *Language* 67:510—546.

Moscati, Sabatino (ed.)
1964 *An Introduction to the Comparative Grammar of the Semitic Languages.* Wiesbaden: Otto Harrassowitz.

Mous, Maarten
2012 "Cushitic", in: Zygmunt Frajzyngier and Erin Shay (eds.), *The Afroasiatic Languages*. Cambridge: Cambridge University Press, pp. 342—422.

Nichols, Johanna
1992 *Linguistic Diversity in Space and Time*. Chicago, IL: The University of Chicago Press.

Orël, Vladimir E. and Olga V. Stolbova
1995 *Hamito-Semitic Etymological Dictionary: Materials for a Reconstruction*. Leiden: E. J. Brill.

Pedersen, Holger
1931 *The Discovery of Language: Linguistic Science in the Nineteenth Century.* English translation by John Webster Spargo. Midland book edition 1962. Bloomington, IN: Indiana University Press.

Pereltsvaig, Asya
2012 *Languages of the World: An Introduction*. Cambridge: Cambridge University Press.

Poppe, Nicholas [Nikolaus]

1955 *Introduction to Mongolian Comparative Studies*. Reprinted 1987. Helsinki: Suomalais-Ugrilainen Seura.

1960 *Vergleichende Grammatik der altäischen Sprachen [Comparative Grammar of the Altaic Languages].* Teil I: Vergleichende Lautlehre. Wiesbaden: Otto Harrassowitz.

1965 *Introduction to Altaic Linguistics.* Wiesbaden: Harrassowitz.

Ramat, Anna Giacalone and Paolo Ramat (eds.)

1998 *The Indo-European Languages*. Translated from *Le Lingue Indoeuropee [The Indo-European Languages]* (1993). London and New York, NY: Routledge.

Ramstedt, G[ustav] J[ohn]

1952—1957 *Einführung in die altäische Sprachwissenschaft [Introduction to Altaic Linguistics].* 2 vols. Helsinki: Suomalais-Ugrilainen Seura.

Raun, Alo

1988 "Proto-Uralic Comparative-Historical Morphosyntax", in: Denis Sinor (ed.), *The Uralic Languages*. Leiden: E. J. Brill, pp. 555—571.

Reiner, Erica

1969 "The Elamite Language", in: B. Spuler (ed.), *Altkleinasiatische Sprachen [Ancient Near Eastern Languages]*. Leiden: E. J. Brill, pp. 54—118.

Renfrew, Colin and Daniel Nettle (eds.)

1999 *Nostratic: Examining a Linguistic Macrofamily*. Cambridge: McDonald Institute for Archaeological Research.

Rix, Helmut

1992 *Historische Grammatik des Griechischen: Laut- und Formenlehre [Historical Grammar of Greek: Phonology and Morphology]*. 2nd edition. Darmstadt: Wissenschaftliche Buchgesellschaft.

Róna-Tas, A.

1991 *An Introduction to Turkology*. Szeged: Attila József University.

1998 "The Reconstruction of Proto-Turkic and the Genetic Question", in: Lars Johanson and Éva Ágnes Csató (eds.), *The Turkic Languages*. London and New York, NY: Routledge, pp. 67—80.

Rössler, Otto

1981 "The Structure and Inflexion of the Verb in the Semito-Hamitic Languages: Preliminary Studies for a Comparative Semito-Hamitic Grammar] (translated by Yoël L. Arbeitman) in: Yoël L. Arbeitman and Allan R. Bomhard (eds.), *Bono Homini Donum: Essays in Historical Linguistics in Memory of J. Alexander Kerns*. Amsterdam: John Benjamins, part II, pp. 679—748.

Rubin, Aaron D.
2010 *A Brief Introduction to the Semitic Languages*. Piscataway, NJ: Gorgias Press.
Sasse, Hans-Jürgen
1984 "Case in Cushitic, Semitic, and Berber", in: James Bynon (ed.), *Current Progress in Afro-Asiatic Languages: Papers of the Third International Hamito-Semitic Congress*. Amsterdam and Philadelphia, PA: John Benjamins, pp. 111—126.
Schmalstieg, William R.
1980 *Indo-European Linguistics: A New Synthesis*. University Park, PA, and London: The Pennsylvania State University Press.
Schmidt, Karl Horst
1979a "Probleme des relativen Chronologie" [Problems of Relative Chronology], *Incontri Linguistici* 5:55—59.
1979b "Zur Vorgesichte des indogermanischen Genussystems" [On the Prehistory of the Indo-European Gender System], in: Bela Brogyanyí (ed.), *Studies in Diachronic, Synchronic and Typological Linguistics: Festschrift for Oswald Szemerényi*. Amsterdam: John Benjamins, part II, pp. 793—800.
1980 "Zur Typologie des Vorindogermanischen" [On the Typology of Pre-Indo-European], in: Paolo Ramat (ed.), *Indo-European Reconstruction and Indo-European Syntax: Proceedings of the Colloquium of the "Indogermanische Gesellschaft", University of Pavia, 6—7 September 1979*. Amsterdam: John Benjamins, pp. 91—112.
Schmitt-Brandt, Robert
1967 *Die Entwicklung des indogermanischen Vokalsystems [The Development of the Indo-European Vowel System]*. Heidelberg: Julius Groos.
[1973] [2nd edition.]
1998 *Einführung in die Indogermanistik [Introduction to Indo-European Studies]*. Tübingen: Francke Verlag.
Schwink, Frederick W.
1994 *Linguistic Typology, Universality and the Realism of Reconstruction.* Washington, DC: Institute for the Study of Man.
Shevoroshkin, Vitaly (ed.)
1989a *Explorations in Language Macrofamilies.* Bochum: Universitätsverlag Dr. Norbert Brockmeyer.
1989b *Reconstructing Languages and Cultures.* Bochum: Universitätsverlag Dr. Norbert Brockmeyer.

1990 *Proto-Languages and Proto-Cultures.* Bochum: Universitätsverlag Dr. Norbert Brockmeyer.
1991 *Dene-Sino-Caucasian Languages.* Bochum: Universitätsverlag Dr. Norbert Brockmeyer.

Shevoroshkin, Vitaly and Thomas L. Markey, Thomas L. (eds.)
1986 *Typology, Relationship, and Time.* Ann Arbor, MI: Karoma.

Shields, Kenneth
1982 *Indo-European Noun Inflection.* University Park, PA: The Pennsylvania State University Press.
1992 *A History of Indo-European Verb Morphology.* Amsterdam and Philadelphia, PA: John Benjamins.

Sihler, Andrew L.
1995 *New Comparative Grammar of Greek and Latin.* New York, NY, and Oxford: Oxford University Press.

Sinor, Denis
1988 "The Problem of the Ural-Altaic Relationship", in: Denis Sinor (ed.), *The Uralic Languages: Description, History and Foreign Influences.* Leiden: E. J. Brill, pp. 706—741.
1990 *Essays in Comparative Altaic Linguistics.* Bloomington, IN: Research Institute for Inner Asian Studies.

Sinor, Denis (ed.)
1988 *The Uralic Languages: Description, History and Foreign Influences.* (= *Handbuch des Orientalistik, VIII, Abt., vol. 1.*) Leiden: E. J. Brill.

Speiser, Ephraim A.
1941 *Introduction to Hurrian.* New Haven, CT: American Schools of Oriental Research.

Starostin, Sergej A.
1991 *Алтайская проблема и происхождение японского языка [The Altaic Problem and the Origin of the Japanese Language].* Moscow: Nauka.
1999 "Subgrouping of Nostratic: Comments on Aharon Dolgopolsky's *The Nostratic Macrofamily and Linguistic Palaeontology*", in: Colin Renfrew and Daniel Nettle (eds.), *Nostratic: Examining a Linguistic Macrofamily.* Cambridge: McDonald Institute for Archaeological Research, pp. 137—156.

Starostin, Sergej A., Anna Dybo, and Oleg A. Mudrak
2003 *An Etymological Dictionary of Altaic Languages.* 3 vols. Leiden: E. J. Brill.

Steever, Sanford B.
1998 "Introduction to the Dravidian Languages", in: Stanford B. Steever (ed.), *The Dravidian Languages*. London and New York, NY: Routledge, pp. 1—39.
Steever, Sanford B. (ed.)
1998 *The Dravidian Languages*. London and New York, NY: Routledge.
Stempel, Reinhard
1999 *Abriß einer historischen Grammatik der semitischen Sprachen [Outline of a Historical Grammar of the Semitic Languages]*. Frankfurt am Main: Peter Lang.
Sturtevant, Edgar H.
1942 *The Indo-Hittite Laryngeals*. Baltimore, MD: Linguistic Society of America.
Szemerényi, Oswald
1996 *Introduction to Indo-European Linguistics*. Translated from the 4th edition (1990) of *Einführung in die vergleichende Sprachwissenschaft [Introduction to Comparative Linguistics]*, with additional notes and references. Oxford: Clarendon Press.
Takács, Gábor
2011 *Studies in Afro-Asiatic Comparative Phonology: Consonants*. Berlin: Dietrich Reimer Verlag.
Trask, R. L.
1993 *A Dictionary of Phonetics and Phonology*. London and New York, NY: Routledge.
Van Windekens, A[lbert] J[oris]
1944 *Morphologie comparée du tokharien [Comparative Morphology of Tocharian]*. Louvain: Le Muséon.
1976—1982 *Le tokharien confronté avec les autres langues indo-européennes [Tocharian Compared with the Other Indo-European Languages]*. 3 vols. Louvain: Centre International de Dialectologie Générale.
Villar, Francisco
1991 *Los indoeuropeos y los orígenes de Europa: Lenguaje e historia [The Indo-Europeans and the Origins of Europe: Language and History]*. Madrid: Gredos.
[1996] [2nd edition.]
Watkins, Calvert
1962 *Indo-European Origins of the Celtic Verb. I. The Sigmatic Aorist*. Dublin: The Dublin Institute for Advanced Studies.

1998 "Proto-Indo-European: Comparison and Reconstruction", in: Anna Giacalone Ramat and Paolo Ramat (eds.), *The Indo-European Languages*. London and New York, NY: Routledge, pp. 25—73.

Wegner, Ilse
1999 *Introduction to the Hurrian Language*. English translation by the author of the first edition of *Einführung in die hurritische Sprache*. Unpublished manuscript.
2007 *Einführung in die hurritische Sprache [Introduction to the Hurrian Language]*. 2nd edition. Wiesbaden: Otto Harrassowitz Verlag.

Wilhelm, Gernot
2004a "Hurrian", in: Roger D. Woodard (ed.), *The Cambridge Encyclopedia of the World's Ancient Languages*. Cambridge: Cambridge University Press, pp. 95—118.
2004b "Urartian", in: Roger D. Woodard (ed.), *The Cambridge Encyclopedia of the World's Ancient Languages*. Cambridge: Cambridge University Press, pp. 119—137.

Zaborski, Andrzej
1984 "Remarks on the Genetic Classification and the Relative Chronology of the Cushitic Languages", in: James Bynon (ed.), *Current Progress in Afro-Asiatic Languages: Papers of the Third International Hamito-Semitic Congress*. Amsterdam and Philadelphia, PA: John Benjamins, pp. 127—138.
1990 "Preliminary Remarks on Case Morphemes in Omotic", in: Richard J. Hayward (ed.), *Omotic Language Studies*. London: University of London, School of Oriental and African Studies, pp. 617—639.

Zvelebil, Kamil V.
1970 *Comparative Dravidian Phonology*. The Hague: Mouton.
1977 *A Sketch of Comparative Dravidian Morphology*. Part I. The Hague: Mouton.
1990 *Dravidian Linguistics: An Introduction*. Pondicherry: Institute of Linguistics and Culture.

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

The Early Dispersions of *Homo sapiens sapiens* and proto-Human from Africa

Harold C. Fleming[#], Stephen L. Zegura[&], James B. Harrod[@],
John D. Bengtson[§], Shomarka O.Y. Keita[+]

Department of Anthropology and African Studies Center, Boston University, Boston, Mass.[#]
Department of Anthropology, University of Arizona, Tucson, Az.[&]
Maine College of Art, Portland, Maine[@]
Association for the Study of Language in Prehistory, Harvard Square, Cambridge, Mass. [§]
Department of Anthropology, Smithsonian Institution and Howard University, Washington, D.C.[+]

Abstract

Against a background of intermittent faunal exchanges between Africa and Eurasia over millions of years, we argue that evidence from three sub-fields of Anthropology point to two dispersions of *Homo sapiens sapiens* and human language out of Africa. The **first** dispersion clustered around 100 kya and clearly associated with the Middle Stone Age, probably settled most of tropical Eurasia and most of insular Southeast Asia, reaching Australia and New Guinea around 60 kya. **No dispersion north of India happened**, the human advance being confined to south Eurasia and the tropics up to Melanesia. Contact and inter-breeding with Neanderthals and their cousins, the Denisovans, probably slowed or confined the human progress beyond the tropics.

Then, we argue that a **second major** dispersion occurred around 50 ky later, often called the "**Aurignacian**" and associated with the linguistic **phyletic chain called Borean**, This second dispersion from Africa was the source of the Upper Paleolithic in Eurasia and most modern languages of Eurasia and all in the Americas.

Oriented around the 'four fields' model of historical anthropology, the disciplines involved were biological anthropology, archeology, and historical linguistics.

* * * *

Africans of the Interglacial period of ~100,000 to 130,000 BP began leaving the Horn of Africa to settle other regions of Africa and southwestern Asia, and ultimately southern or tropical Eurasia, Sundaland, Papua, Melanesia, and Australia. These early migrants were speaking **early varieties of human language**, genetically related to those which came after them, as well as to those ancestral to them. While there may have been more major dispersions during these early periods, we focus on just one **multi-faceted** and **reasonably well-documented** dispersion. A second dispersion came later around 50,000 BP which accomplished the settlement of Europe, the rest of Asia and the New World, as well as providing the ancestors for all modern languages of Europe, the New World, and most of Eurasia.

Our dyadic dispersal hypothesis not only accounts for language origins which have probably never been proposed before but basically replaces the current vague consensus that the period around 50,000 BP saw the first movement of language-using humans out of Africa. The stimulating theory, proposed by Klein **(1)**,[1] which bundled 'fully modern' physiques with symbolic behavior, especially art, and with language, did not survive the recent explosion of archeological studies with much earlier dates for the egress of *Homo sapiens sapiens* from Africa. Moreover it was wounded by the critical responses **(2)** to the notion of "fully modern" and therefore symbolic behavior being denied to such early finds as Qafzeh of ~100,000.

Since our approach rests on the so-called "four fields" model, we need to stipulate how each sub-field of anthropology furnished, and furnishes, support for our basic hypothesis. Because the discipline of Physical Anthropology (Biological Anthropology) supports us in three distinct ways we shall begin with it.

Paleoanthropology: Fossil studies not only present the evolutionary antecedents of modern man, e.g., *Australopithecus, Homo habilis, Homo erectus*, etc., they also document the presence of specific discoveries of *Homo sapiens sapiens*, our own sub-species **(3)**, but also our sometime competitor *Homo sapiens neanderthalensis (*hereinafter *Neanderthal)* in various parts of the world at various dates. Fossil studies have documented the presence of *Homo sapiens sapiens* (hereinafter *H.s.s.*) during the time period 150,000 to 50,000 in the following areas: southern Africa, Tanzania, Ethiopia-Eritrea (hereinafter Ethiopia), North Africa, southwest Asia, and Southeast Asia. Sites include Klasies River Mouth, Border Cave, Mumba, Lake Natron (footprints), Dire-Dawa, Herto, Ifri n'Ammer, Témara, Skhul-B & Qafzeh, and Zhirendong (or Zhiren Cave). The distribution appears to be rooted in Ethiopia **(4)**, which is the sole possessor of a progression of *H.s.s.* fossils from 195 kya to 125 kya, as well as continuity in Middle Stone Age tool kits in the same period. Given this evidence, it is highly unlikely that *H.s.s.* of this time period was found in non-tropical Eurasia or the New World. (Falk, *et a*l., 2005, 2007)

Outside of the time period but highly relevant to the hypothesis are the Flores Island (Indonesia) fossils, now seen as probably not *H.s.s.* (Falk, *et al.*, 2005, 2007). Molecular bio-genetic sampling of the **modern** Flores islanders indicates that their mtDNA haplotypes are common in the Austronesian realm, while their Y chromosomes are more like mainland Austric. However Reich, *et al.*, (2010) found *Denisovan* genetic material in modern Flores islanders. One is inspired to speculate about the possible *Denisovan* content of the fossil Flores islanders! Might the 'hobbits' have been *Denisovan*s? (See molecular anthropology section.)

Human taxonomy: Classification was once a major concern of Physical Anthropology. Nowadays, however, because of the cultural warfare over the concept of "race" and bio-genetic criticisms of classifications based on phenotypes rather than genotypes, taxonomy does not enjoy its former prominence. However, there are modern attempts at taxonomy which rely predominantly on non-DNA genotypic data for drawing taxonomic inferences.

---

[1] The **bold numerals** in parentheses refer to **End Notes**, at the end of the article.

Cavalli-Sforza and his colleagues (1994) sampled a world-wide range of populations from all continents and most isolates, including searching most of the literature. They calculated genotypes from phenotypic data on the following range of genetic systems: ABO, Rhesus, GammaGlobulin, KEL, Duffy, MNS, LE, LU, P1, PEPA, B, and C; PTC, PGM1 and 2, PGD, PGK1, FUT2(SE), AOD1, TF, ACP1, ADA, AK1, ALPP, AG, LPA, CP, CHE1 and 2, C3, D1, ESD, G6PD, BF, GLO1, GC, HP, JK, LDH, and HLAA and HLAB (with 12 and 17 variants, respectively). These 40 loci nearly exhausted the universe of taxonomically useful genes before molecular DNA research took over.

Among their findings: (a) all modern peoples are related to each other before they are related to other taxa, including the great apes and *Neanderthal.* (b) African peoples form a bio-genetic cluster distinct from the rest of humanity. (c) the likely derivation of non-African peoples is from Africa, or Africa must be the homeland for humanity. Cavalli-Sforza and his colleagues in their final classification of bio-genetic (non-DNA) evidence divided humanity into **Africans** and **non-Africans** and **t**hen non-Africans into **Northeurasians** and **Southeastasians**. Then **Northeurasians** separate into **'Caucasoids'** (including north Africans) and **'Mongoloids'** (including Amerinds) and then **Southeastasians** divide into **Mainland & Insular** versus **Papuans & Australians** where **Mainland** equals mainland Southeast Asia, which explicitly includes most of Indonesia and the Philippines, but also south China and northeast India.

Insular means **Melanesian, Micronesian,** and **Polynesian** island populations and many of the peoples of the northern coasts of New Guinea. While traditional or earlier racial classifications had linked the Southeast Asians to the Mongoloids, perhaps due to the massive effects of Chinese civilization on the area, Cavalli-Sforza, *et al*., (1994) showed that a more complex relationship was actually the case. Finally, **Papuans** and **Australians** referred to the autochthones of New Guinea and Australia + Tasmania.

Also supplementing this synthesis in one broad region was a large scale classical (Gamma Globulin) study which found one haplotype ***fanb*** almost perfectly correlated with the world distribution of *Austric* languages, including in eastern India, but much diminished in north China, Korea, Japan, and Tibet. It appeared slightly in lower caste groups and/or 'tribals' in central India, but was absent in Australia, and nearly absent in most of central and southern New Guinea. Its highest percentages and thus its probable roots lay in mainland Southeast Asia. See Steinberg and Cook (1981). Their research on Gamma Globulin was worldwide in scope. The Austric ***fanb*** pattern was particularly striking, as was ***fb*** which was centered on Anatolia but included all of Europe and the Middle East. It included North Africa, Arabs of the Sahara, Egypt, Ethiopia, Somalia, and such isolates in East Africa as the Tutsi and Iraqw (Tanzania). Most of India, either as whole populations or as upper caste groups in the east and south, showed ***fb.*** The correlations with Cavalli-Sforza, *et al.*'s (1994) taxa were striking.

Equally striking, perhaps, are the potential links between some of their clusters and the major dispersions proposed herein, as well as the major language super-phyla.

Dispersion One, for example, when completed, had a natural geographical correlation with four great tropical Eurasiatic and Oceanic language groups, viz. *Australian, Papuan, Paleo-Sundic,* and *Austric.* **Dispersion Two** links Cavalli-Sforza, *et al.*'s (1994) Northeurasians to the Borean 'phyletic chain' and most of Earth's northern Hemisphere.

Molecular Anthropology: This is the newest of Biological Anthropology's contributions to prehistory. It is not unique to Anthropology, having its origins in Biology and being now used extensively in medical research. There are even popular television shows which use bio-genetics to explore the specific histories and genealogies of individual citizens. .

DNA-based research has not been mainly interested in overall human taxonomy, although some rather large regional classifications have been done. Besides establishing the separateness of modern humans from kindred primates such as chimpanzees, gorillas, and orangutans, studies of fossil bone genomes concluded that the *Neanderthals* were also distinct, albeit closer to us than the other primates. Most recently, another possible sub-species of *Homo sapiens* from Denisova Cave was discovered, albeit lacking any skeletal evidence save one finger bone and two molar teeth: it might be an east Eurasian counterpart of Neanderthal (Meyer, *et al*., 2012)

From the standpoint of our hypothesis one of the most important DNA studies was one of the earliest (Cann, *et al.,* 1987). Based entirely on mitochondrial DNA (mtDNA), it proposed three important things. **First**, the probable date for "mitochondrial" EVE, the hypothetical mother of humanity or at least her mtDNA, was around 200 kya**. (5)**

Secondly, Africans were a moiety of humanity, i.e., the rest of the world's humans were the other "half" of mankind. Or as others put it recently "African populations show the greatest genetic diversity, with genetic variation in Eurasia, Oceania, and the Americas largely being a subset of the African diversity…with limited contribution from archaic humans…" (Schlebusch, *et al*., 2012). **Third**, the outside moiety or non-Africans were derived from Africa.

From this it followed, of course, that non-Africans left Africa sometime after 200 kya (the time of the mitochondrial EVE population) which dictated the time period when their descendants – *H.s.s.* – could move around in Africa and migrate to Eurasia. Basically, this scheme was very similar to the one produced by Cavalli-Sforza and his colleagues (1994). The roots of the belief that Africa is humanity's homeland probably go back to Darwin, at least.

Another important and wide-ranging DNA-based study (see Tishkoff and Williams, 2002) made the following conjectures:

(a) earliest common human at 200 kya;
(b) beginning diversification among Africans around 150 kya;
(c) dividing of Ethiopians + non-Africans from the other Africans around 125 kya;
(d) a split between Ethiopians ("Northeast Africans") and non-Africans at 100 kya;
(e) a budding off of Oceanians circa 85 kya (from Eurasians);
(f) a split between Europeans and Asians at 45 kya, possibly in Kazakhstan;
(g) Asians and Americans diverge around 32 kya, probably in eastern Siberia.

This study in general is congruent with our dispersal scenarios. A more recent study (see Tishkoff, *et al.,* 2009) postulates an original human homeland around the Namibia-Angola border, despite the archeological evidence of an Ethiopian origin. Yet they do propose a center for later dispersions out of Africa *circa* 37.5° E by 22.5° N or "near the midpoint of the Red Sea." That point is not far from the Eritrean archeological site of Abdur Reef, which we postulate as important in the first dispersion of *H.s.s.* from Africa. One difference between Tishkoff and Williams (2002) (T&W) *versus* Cavalli-Sforza, *et al.*, (1994) is that T&W put Ethiopians in a moiety with non-Africans, rather than in a moiety with Bushmen. Their conclusion (b) about beginning diversification among Africans *circa* 150 kya is dramatically supported by the Cruciani, *et al.*,'s (2011) judgment that the coalescence time of 'male specific' Y chromosome (MSY) lineages in Africa is 142 kya. Although surprising in view of the Omo-Kibish date of 195 kya, or T&W's own 200 kya for common human, the MSY dates with lineages in central and northwest Africa definitely suggest the **Aterian** archeological sites.

One of T&W's conclusions is that "populations in northeast Africa might have diverged from the rest of sub-Saharan Africa early in the history of modern African populations and that a subset of this northeast African population migrated out of Africa and populated the rest of the globe. Analysis of mtDNA and Y chromosome diversity supports a single East African source of migration out of Africa." Also see Hammer, *et al.*, (1998) for movement of Y chromosomes (and therefore males) both out of and back into Africa.

Still Cavalli-Sforza and colleagues in 1994 proposed a closer relationship between Ethiopians and Bushmen among Africans as far as "classical" or non-molecular genetic factors were concerned, while Tishkoff, *et al.,* (2009) proposed an early link between South African Khoisan and Pigmies. The *Khoisan*-speaking Hadza of Tanzania were found to be almost uniquely isolated, while doubts were raised about their linguistic status as members of Khoisan. We do not share those doubts but do believe that *Hadza* is the most divergent member of Khoisan. Yet there are puzzling things about the Bushmen and the *Khoisan* language phylum. Let it suffice for now to say that the obvious age of modern humans, usually called Bushmen, in southern Africa seems much older than the probable age of Khoisan. Also the poorly known "Berg Dama" of southwest Africa speak local *Khoisan* and *Bantu* languages yet are quite distinct physically from their Bantu and Khoisan neighbors.

T&W's point (g), the divergence between **Asians and Americans** at 32 kya, finds support in the archeological site of Yana River of around 30 kya in eastern Siberia. This site provides a date for Amerinds to begin their dispersion into North America. Despite the steadfast refusal of some leading archeologists to abandon the Clovis horizon, roughly 12.9-13.2 kya, as the first level of settlement, geneticists have been consistently proposing earlier dates, generally 15-18 kya.

Another regional molecular study by Chu, *et al.,* (1998) presented research on a number of Southeast Asian and Chinese populations and rooted their ancestral area in Southeast Asia. Most of the mainland Southeast Asian distribution of the *Austric* (linguistic) super-phylum falls within the purview of the populations sampled by Chu, *et* *al.*, (1998). The homeland of that super-phylum almost certainly lies in mainland Southeast Asia, with Burma being the leading candidate. However, the presence of Nihali and Munda in India argues for a very respectable antiquity for Austric, west of Burma.

A more recent and much larger Single Nucleotide Polymorphism (SNP) study, involving 93 scholars from 40 institutions and thousands of genetic markers, focused on 73 east and Southeast Asian populations but also included a few European and African populations for comparative purposes (Abdulla, *et al*., 2009) It screened each sample for more than 50,000 (SNPs) sites on chromosomes where a single base can vary from one individual to another. The number of variations, presented as different haplotypes, indicates how closely related two individuals are genetically. Not surprisingly, the genetic groupings correlate with linguistic and geographic groupings. But the consortium also found that genetic diversity markedly decreased going from south to north. In addition, most of the genetic variations found in East Asian populations were also present in the Southeast Asian populations, indicating that the former likely derived from the latter. The authors conclude that humans migrated along a coastal route from the Mideast to Southeast Asia and from there moved north, gradually adapting to harsher climates.

Moreover, this study concludes that both Negrito and non-Negrito populations derive from a "single primary wave of entry of humans into the continent." Basically, this study is a confirmation of Chu, *et al*., (1998). However, two other studies (Karafet, *et al*., 2001, Zhong, *et al*., 2011) conclude that North Asia was populated both from Southeast Asia and from Central Asia and that there was also some north to south migration, thereby underscoring the complexity of the peopling of Asia.

Two assertions in the SNP study require comment. There is nothing in the data which requires that a 'coastal route' be followed or that it derive from the Mid-East. Since an ultimate African origin is assumed, the people could have come in four different ways. Either by the south Arabian coast to India, directly by sea from Somalia to India, from the Levant via the Persian Gulf, or from Africa by sea to Indonesia, thence to Malaya. The fourth alternative is most difficult and easily rejected, but the second is not, being the ancient and well known Indian Ocean trade route.

A more serious objection can be made to their combining '*Sino-Tibetan*' populations with '*Tai-Kadai*' populations in a 'major linguistic group', which contrasts with such as '*Altaic*', '*Hmong-Mien*' (or *Miao-Yao*), and others. *Sino-Tibetan* as a phylum is only **very distantly** related (back in proto-human) to *Tai-Kadai* (or *Daic*) as a phylum. While *Tai-Kadai* is most likely a member of *Austric*, along with *Hmong-Mien*, *Sino-Tibetan* is **certainly not**. It is either a member of *Sino-Caucasic* (Starostin, 1988) or before that a component of the second major dispersion, as listed in the *Borean* hypothesis at the end of this paper. The *Thai* relationship to *Chinese* seemed obvious to earlier linguists, but it was due to the numerous loan words which passed between them.

In Africa, itself, recent studies contributed to the puzzling prehistory of the physically distinctive Pigmies of the central African forests (Verdu, *et al*., 2009). The analysis of Bantu foresters vis-à-vis Pigmies in central Africa was an especially informative local DNA study. Not only did they determine the structure of intermarriage between the groups but also, despite regional coexistence and cooperation, Bantus and

Pigmies remained distinct. Finally, theories about Pigmies simply being Bantus with glandular problems to explain their small size were vitiated by the proposed great time depth between the two populations. Thus, we may suggest another dispersion within Africa — to the west — with *H.s.s.* leaving forested Ethiopia to go west into the Congo forest, a mere 500 miles from the Omo, or onto the Sudanese savannahs.

Ancient DNA studies have recently provided surprising evidence of two related allotaxa of *Homo* with whom *H.s.s.* interbred to a limited extent during the early dispersions into Eurasia. Denisova Cave in the Altai yielded a finger which was analyzed for DNA and found to be distinct from either *H.s.s.* or *Neanderthal.* Recent research has found that Denisovan contributions to the genome are limited to New Guinea, Australia, the Philippines, Polynesia, Fiji, and eastern Indonesia. They are not found in western Indonesia, East Asians, and specific ethnic groups in Malaysia or the Andaman Islands. Contact with *Denisovans* is attested by the 4-6% of *Denisovan* genetic material being found in Papuans of Melanesia. The contact zone with *Denisovans* is most likely to have been in Southeast Asia. Thus *Denisovan*s may have had a distribution comparable to *Neanderthals*, covering a major part of eastern Eurasia when first encountered by *H.s.s.* (Reich, *et al.,* 2010, 2011; Meyer, *et al*., 2012; Green, *et al.,* 2008, 2010). Very recently, genetic evidence of yet another hominin group has been discovered in the same Denisova cave. Traces of its enigmatic genome were found in a Neanderthal specimen and this heretofore unknown group seems to have interbred with the Denisovans (Birney & Pritchard, 2013).

At the same time it is clear that *Neanderthal*, whose interactions with their sister group, the *Denisovan*s, are unknown, did interbreed with *H.s.s.* Contributions to the human genome by Neanderthals average about 2% for non-African populations, while Africans have no detectable Neanderthal ancestry (Birney & Pritchard, 2013). Recent evidence of contributions to the genome of Amerinds and East Asians implicates Neanderthals rather than Denisovans (Reich, *et al*., 2012).

Archeology: Because the discipline of Archeology supports us in clear ways we will turn to it next. **Archeology** presents well-dated sites with fossil data, material culture, evidence of tool kits, food, clothing, housing, social structure, and sometimes evidence of symbolic behavior and, with luck, music and language. An important non-cultural facet of archeological sites is the evidence they provide on the ecological setting and the climate prevailing in the area of the site. In the time period in question, archeological sites **(6)** relevant to the problem have been found in southern Africa, Ethiopia, Nubia, north Africa, the Levant, southern and eastern Arabia, western India, and Australia in a later time period.

Generally, the time period (interglacial) represented less rainfall tied up in glaciers and more water in lakes, rivers, and seas. While the glacial period which followed – 70 kya to 11.6 kya – saw severe drought conditions in many parts of Africa, some populations survived and some probably moved elsewhere. During the penultimate interglacial period, there also was a great deal of movement.

Material culture of *H.s.s.* was similar to that of *Neanderthal* and was often labeled **Mousterian**, particularly in those areas closest to *Neanderthal* regions like southwest Asia (*e.g.,* Qafzeh in Israel). This may be the result of culture contact between *Neanderthal* and

H.s.s. However, much of North Africa had a similar tool kit usually labeled as **Aterian.** Yet the reports on eastern Arabia, (Jebel Faya in United Arab Emirates), assert a difference between the Qafzeh tool kit and that of Jebel Faya whose affinities supposedly lie more with Ethiopia than with the Levant. At many Aterian sites, large molars were noted. Large tooth size is also characteristic of native Australians but not of modern Europeans, although one early site in Romania (Oase) of 40 kya did have them. Teeth at Qafzeh and Skhul were similarly large.

Fundamentally, **one** of our two dispersions falls within the **Middle Stone Age** of Africa (**MSA**), while the second is coterminous with the **Late Stone Age** of Africa (**LSA**). Outside of Africa the MSA is called the **Middle Paleolithic** (**MP**) and the LSA is called the **Upper Paleolithic** (**UP**). In Europe and western Asia the MP is largely associated with *Neanderthal*, while in Africa it is **not**. There it is **associated with *H.s.s.*** In eastern Asia an indeterminate amount of territory is probably associated with the *Denisovans*. Except for one cave in the Altai, the *Denisovans* are devoid of archeological attestations; they are known almost exclusively from bio-genetic data and analyses. Since both Eurasian hominin allotaxa have interbred with *H.s.s.*, the locations and times of such genetic exchanges are very pertinent to the pre-history of *H.s.s.* dispersals outside of Africa. The fact that the hemispherically remote Amerinds of the Americas have traces of *Neanderthal* genes, but not *Denisovan* genes, speaks volumes about the complex Eurasian pre-history of the American autochthones.

When the dispersions of the several species/sub-species of *Homo* in Africa and Asia are discussed, it is usually the case that the previous inhabitants are not mentioned. Yet sometimes they are relevant because they could easily affect the speed or ease with which the humans advanced. Following Day (1986, p.417), we can assume that the most likely prior populations were of *Homo erectus* (in Eurasia) or *Homo heidelbergensis (Africa)*, wherein species interbreeding may have been possible but no bio-genetic traces are found. Moreover the concept of **"archaic"** *H.s.s.*, versus "**modern**" (AMH) has seriously affected analysis because such sites as Qafzeh were not included for reason of being "archaic", i.e., failing to be AMH. (See Klein, 1999 and Oppenheimer, 2009.) If there were populations of "archaic" *H.s.s.*, then one would expect that resistance would be much greater and conquest or replacement much more difficult. Presumably **interbreeding** would become much **more likely** with probable differences in the impact on **mtDNA** and **Y** chromosome frequencies. (See Forster and Renfrew, 2012)

We have used the term 'dispersion' instead of 'migration' to label the early movements of *H.s.s.* towards the north and east. **(7)** Three separable or distinctive aspects of one basic archeological horizon are discernible. The **first** discernible movement (Aterian) is associated with North Africa and later the Sahara. It could be derived from a movement from the Sudan up the Nile, thence west to the Maghreb. However, it may have moved through Saharan wet phases via Lake Chad to the Maghreb. It shows no signs of contact with Neanderthal (see later), even though one Aterian site is found in Israel, and its presence in the Sahara was eliminated by the severe aridity of the last glacial period (70 kya to 11.6 kya). We consider Aterian largely an intra-African movement but possibly the **first movement out of Africa**.

Recent proposals (Scerri, 2012; Hublin & Klein, 2012) have de-emphasized the **tanged point** aspect of Aterian, named after the **Bir el Ater** site in Algeria, and emphasized the general MSA similarities in tool kits from remote northwestern Maghreb (e.g., Contrebandiers or **Smuggler's Cave**, Morocco) across North Africa to Egypt (**Kharga Oasis, Bir Tarfawi**) and the Sudan (**Sai Island**) and extending into the Levant (Hublin and Klein, 2012). Scerri's (2012) conclusion is that "The Aterian is not a discrete chronostratigraphic unit and tanged points cannot serve as a main criterion for the definition of an 'Aterian' complex." Also using improved dating methods, Scerri (2012) reckons that most Aterian sites in North Africa occur in the **Marine Isotope Stage (MIS)** 5 and 6 or a range from ~**145 ± 9 kya** at **Ifri n 'Ammar** to **61 ± 10 kya** at **Uan Tabu** (Libya). Older methods yielded a **150 kya** at the Saharan site of **Adrar Bous** in northern Niger, closer to the Sahel and Lake Chad than to the northern sites. Most 'Aterian' sites hover around the **100 ± 10 ka** range. Scerri (2012) also conjectured that the peoples were ultimately of sub-Saharan origin. In all the comparisons, **Haua Fteah** of Cyrenaica stood out as exceptional and led to the conclusion that the people of Haua Fteah were different from the other 'Aterians'. Scerri (2012) proposed that the Egyptian sites overlapped enough with so-called Nubian culture or industrial complex to suggest an eastern focus of 'Aterian' in contrast with a western focus. During periods of greater humidity 'Aterian' consisted of many sites in the western and central Sahara. See below for its proposed links to two major African linguistic phyla.

The second movement went up the Nile or via the Red Sea, and is anchored perhaps at 125 kya at Abdur Reef on the Eritrean coast. This second movement settled in the Levant (Qafzeh, Skhul) in contact with Neanderthals, and probably spread east along the Persian Gulf route to India. This could also be the source of a third facet or variant , sometimes called the "Nubian Complex" which went up the Nile but which also followed the south Arabian coast through Oman to Hormuz, thence to India and ultimately to Southeast Asia. Given the geography of the lower Red Sea area, it would not be surprising to find archeological cultures of northeastern African origin **bifurcating** between the Nile Valley and the close *Tihama* or lowland coast of Yemen and Saudi Arabia.

However, the dating of Layers in Tabun Cave and in **Hayonim Cave** in Israel and **16R Dune** in **India** suggest that a **fourth** movement to the Levant may have occurred much earlier, the dates being 150 kya and 135 kya, respectively. We know little else about this possible dispersion, except the probable earlier contacts with Neanderthal here and possible source of **Skhul** and **Qafzeh** cultures or influences thereupon. If 16R Dune in India is derived from Tabun Cave, it would mark the earliest move to South Asia. There is also a site in central India (**Bhimbetka**) and one in extreme south China (**Zhirendong**) which are rough contemporaries with Skhul and Qafzeh and some levels of **Jebel Faya**. (Liu, *et al.,* 2010).

The archeological sites are very important to our hypothesis. It is understood that, because of the amounts of time involved, not all the traits or attributes of an earlier site will be continued in its descendants or younger relatives. It is well understood in archeology that changes in environment may lead to changes in tool kits or other facets of culture. There are three things which we expect to show some continuity or which simply need to be noted, viz., presence of symbolic behavior, however measured or defined, specification of different *H.s.s.* populations, and some continuity or resemblances with other older or younger cultures in the technological tradition, e.g., MSA (African), or UP or Levallois, a traditional technique in working stone. In many cases only the third of our expectations can be found.

Not all of these features may be present in any given site; this is due in part to the nature of archeological sampling. Unlike the case in bio-genetics or linguistics where the population in question is often known and a sample is a calculated percentage of the whole, in fossil studies and archeology the sample is "whatever the cat dragged in." Nature exposes data more or less at random and the job of the excavator is to make sense of those data so as to reconstruct a population and its place in history. Naturally, clever or systematic excavators will anticipate where nature will expose pieces of the past, e.g., the valley of the Awash River in Ethiopia is like a gold mine in its wealth of productive sites.

In general terms the latest archeological research suggests multiple dispersals out of Northeast Africa into Southwest Asia of up to four or five MSA / MP cultures during the last Interglacial period, around OIS 5c-5e or ~98-125 kya. Since two of them are probably "dead ends", while two are close in time and place, with each being widespread, we think that there is one basic dispersion to deal with. It is perhaps most fruitful to conceive of these collectively as a "culture area", as articulated by ethnologists, but one on the move, so to speak, hence a "dispersion" (see End Note 7). The four variants or cultures with their most prominent 'type sites' are, as follows:

As a guide to understanding the vocabulary of **modern archeological dating** systems we offer these translations of contemporary 'acronyms' used to label **various** systems. Thus note that **MIS** stands for **"Marine Isotope Stage"**, **OSL** represents **"Optically Stimulated Luminescence,"** while TL refers to **"Thermoluminescence,"** and **ESR** represents **"Electron Spin Resonance." Radiocarbon dating** is constantly being tweaked, although for the present at least it is understood that it reaches to **40 kya** or maybe **50 kya** before which it is considered unreliable. The other three systems record much older dates and are considered nowadays as among the most accurate dating systems we have. Nevertheless some earlier dating systems such as 'tree ring dating' or calendrical match ups, e.g., with Old Egyptian king lists or Sumerian can be the most accurate of all, albeit more limited in time spans.

I. The MSA or MP, Middle Stone Age (Africa) or Middle Paleolithic (Europe), with handaxes, centripetal Levallois cores, discoids.

(A) **Abdur Reef**, Buri Peninsula, Red Sea Coast, Eritrea. Early MSA with handaxes and flake tools. (TIMS U-series on coral), 4 strata, 115-135 kya or **~125±7 kya**. Elephant, hippo, rhino, bovid, crocodile and oysters. (See Walter, *et al.*, 2000). Bruggemann, *et al.*, (2004) distinguishes 2 distinct tool kits: (a) handaxes of Acheulian type, made from volcanic rock and obsidian, associated with oyster beds (oyster harvesting requiring heavy duty tools) and (b) MSA flakes and blades primarily made from obsidian, mostly in near shore and beach environments and associated with the large mammals and among remains of oyster, giant clams and crab parts, possibly reflecting two tool kits of the same peoples. At later phases, oysters were not abundant and only the flakes and blades occur, associated with bivalves, gastropods, and crustaceans.

Of the marine shell-fishing at the site, Walter, *et al.,* (2000) state "this is the earliest well-dated evidence for human adaptation to a coastal marine environment. This new widespread adaptive strategy may, in part, signal the onset of modern human behavior, which supports an African origin for modern humans by 125 kya ago."

Moreover there is evidence of older exploitation of marine resources in Africa, starting at least 160,000 years ago, in the "intertidal zone" which would be "over-exploited, necessitating continuous extension along the beach. This beach-combing model provides an immediate and continuous motive for unidirectional, linear migration. Evidence of marine exploitation is found at the very earliest occupation sites in Australia and most significantly at the Australian threshold dating to greater than **42 ka** cal. BP in **Jerimalai in East Timor**…" (Oppenheimer 2012, p.771).

(B) **Jebel Faya 1**, Sharjah, U.A.E., Assemblage C, (OSL) mean of three dates ~**112 kya**; eliminating the two outliers, **123 ±10 kya**; small handaxes, thick bifacial foliates, hard hammer blades (no Levantine features); derived from E / NE African *façonnage* to make handaxes and foliates (Armitage *et al.*,2011).

Armitage, *et al*., (2011) state that "Artifacts in eastern Arabia dating to 100,000 years ago imply modern humans left Africa early, as climate fluctuated." Baily (2009) reports that on the coast of the Red Sea Middle Paleolithic artifacts occur in terraces that are above the present waterline and believed to be of the last Interglacial. (J. Rose, 2004a, b, 2007) Finally, in support of these theses, based on archeology and bio-genetics, it was suggested **"that modern humans were present in Arabia and South Asia earlier** than currently believed, and probably coincident with the presence of *Homo sapiens* in the Levant **between *ca.* 130, 000 and 70,000 years ago."**

By apparent chronological sequence, one may infer that the northeast African Abdur industry spread to Arabia. The route could be across the Bab-el-Mandeb or around the northern end of the Red Sea. Archeological surveys indicate the entire coast of the Red Sea has MSA / MP surface sites (e.g., Bailey in Petraglia & Rose, 2009).

One attempt to show that this dispersion was not productive or did not lead to further settlement in India or Southeast Asia is inherently unconvincing because the sheer size of the distribution from northeast Ethiopia to Hormuz or more than 1200 miles with many sites involved is immanently credible. See S. Oppenheimer (2012, pp.778-9).

In addition there is reason to believe that this MSA culture persisted in southern Arabia for a long time. Van Beek, Cole, and Jamme (1964) report the wide distribution and success of an African MSA Levallois-Mousterian in Hadhramaut, east Yemen, some of which was carbon^{14} dated to 5131 BC ± 200 yrs. Even with the probable inaccuracy of the carbon date –as exemplified by the initial youth of Qafzeh dates in Israel which were later deepened by Bar-Yosef – they do not necessarily go back to 90 kya but do suggest considerable antiquity for that industry in Yemen which was found on the surface yet underlies the Neolithic industries in Hadhramaut.

II. Early Nubian Complex, with Nubian Levallois reduction plus bifacial foliates and rare handaxes at only some sites.

(A) Aybut Auwal, southern Oman, Nubian complex, (OSL) ~106±9 kya and 197±9 kya, **weighted mean**: **106.6 ± 6.4 kya, one of 100 sites** in the **Dhofar** region, Nubian Complex (in Africa **~128 to 74 kya**); evidence for the spread of a distinct MSA lithic industry out-of-Africa and across the southern Red Sea sometime in the first half of MIS 5 (Rose, *et al.*, 2011)

(B) Sai Island Levels 1-3, in northern Sudan, (OSL) **<152 ± 10 kya** (technology style) OIS 5 which equals **72-130 kya.**

Given the Rose, *et al.*, (2011) map of Early Nubian sites in Africa, with predominance toward Egypt and not Ethiopia, and including at least one Sinai site, the preponderance of current evidence favors a Sinai route out-of-Africa for the Early Nubian Complex culture, albeit with an extension in southern Arabia.

III. Nile Denticulate Mousterian (also called 'Mousterian K-group), with classical Levallois points; handaxes and foliates. This is a possible 'dead end'.

These sites –although limited in number –suggest another culture also spreading from Africa to Southwest Asia. Because most of them are poorly dated it is difficult to work with them. They should be added to discussions of the *Homo sapiens sapiens* out-of-Africa hypothesis but primarily after more data on them are at hand.

(A) Nazlet Khater NK-2, Lower Nile, Upper Egypt, (geostratigraphy) **~100 kya** (Van Peer, 1998).

(B) Sinai-20 Split Rock Site, Wadi al Madibah, Zarnoq area, central eastern Sinai, about 30 km from Taba on the Gulf of Aqaba, Red Sea, (TL) Lower **84.5 ± 13 ka**, Upper **61.5 ± 8.6 ka** 'closer to Nile Denticulate Mousterian than Middle East Levallois Mousterian', (Kobusiewicz, *et al,*. 2001; Kobusiewicz in Eddy, 1999).

IV. Aterian, as discussed above, with many kinds of symbolic behavior. We only mention two sites.

(A) Ifri n'Ammar (Morocco) (TL) Upper OS, tanged items as well as personal ornaments (shellbeads) 83.3±5.6 kya Lower OS, MSA lacking tanged pieces, **130.0 ±7.8 kya**; Upper OI, tanged items, **earliest** appearance of **tanging, 145±9 kya** (Richter, *et al.,* 2010).

(B) Har Karkom, Negev, Israel, at least 2 sites, HK148b, HK72a, "Aterian", no date (Anati E., 2006 online).

We note that Aterian is excellently situated to be the source of the peculiar distribution of *Niger-Congo* languages, which was once described as "coming down from the Sahara like a squall line" (Kay Williams, remarks at a conference on African languages, 1987). *Niger-Congo* has a very eccentric distribution of branches, with the two most divergent or distinctive being located **thousands of miles apart**, on the Atlantic coast in Senegal and far to the east in Kordofan. It is also possible, given its distribution, that the *Nilo-Saharan* linguistic phylum is derived from the Aterian culture area. A genetic relationship between these two phyla has been proposed by a few linguists, *e.g.* Edgar Gregersen (1972). Murdock (1959) assembled evidence of the priority of "Negroids" in the Sahara before the advent of Berbers and Arabs there and associated them with the Niger-Congo and Nilo-Saharan linguistic phyla.

If Anati (2006) has correctly typed the HK sites as Aterian, then there is evidence for this culture "diffusing" from Africa to Southwest Asia. This possibility needs to be added to the *H.s.s.* out-of-Africa hypothesis, although we view Aterian as basically an intra-African culture. Thus far, no Aterian has been reported from Arabia.

V. **Tabun C Industry** (with many kinds of "modern symbolic behavior", similar in complexity to examples of *Neanderthal* and *H.s.s.* Middle Stone Age sites noted herein).

(A) Tabun Cave, Israel, has multiple layers ranging from ~165 to 220 kya; but C level has **~107 kya (ESR)** and **~122** kya (TL). Then **Hayonim Cave**, Israel (TL) **~150 kya.**

(B) Skhul (TL, U-series, ESR) ~**between 100 and 130 kya** (perforated shell beads; pigments in multiple hues of red, orange, yellow; 10 MNI (Minimum Number of Individuals) *H.s.s.*, including four burials, one with wild boar mandible and apparent grave goods).

(C) Ain Hummal, El Kown Basin, central **Syria** (TL) **98 ± 16** and **128 ± 18 kya** (Hauck, *et al.*, 2011). Note this site is on a MIS 5e paleolake only 50 miles from the Euphrates River.

(D) Qafzeh, **~90 kya** (18 MNI *H.s.s.*, including niche depositions and possible burials, one with fallow deer antler over hands placed on the upper chest; perforated shell beads, some with pigment stains, red, yellow and black and broken Levallois core, triangular in shape with incised mostly parallel stroke marks; at least 84 ochre pieces with some at every level, some working traces.)

It seems no one has yet adequately integrated these Tabun C sites into an out-of-Africa scenario. There is no robust evidence for Tabun C beyond the geographic areas noted; it may be a local southwest Asian phenomenon or only absence of evidence for a more distant dispersal of the industry. Moreover, according to Klein's table (1999, p.430) there are non-trivial differences between dates obtained by ESR and those obtained by TL. Thus Tabun-Level C is **~90-125 kya** by ESR and **~95-150 kya** by TL. Both Skhul and Qafzeh are **~100 kya** by both measures but the TL dating estimates extend to **~110 kya.**

However, it is not likely that material **from** Tabun layers of **165-220 kya to** Qafzeh around **90 kya** to Ras el-Kelb of **52 kya** –a span of **130,000** and **168,000 years** respectively – represents the same culture or even the same tradition. Both Skhul and Qafzeh have become famous as sites for modern humans coming out-of-Africa. If they are merely local phenomena, as the argument goes, how did they get to the Levant in the first place, since they are the first *H.s.s.* found in that area? Both Richard Klein and Michael Day were puzzled by or aware of differences between levels at Tabun and other sites. At least one level at Tabun was diagnosed by Day as *Neanderthal* with three of the top four levels labeled as Levalloiso-Mousterian, suggesting *H.s.s.* or *Neanderthal*, while **levels 5 and 6** contained **Acheulian** hand axes, a product probably of *Homo erectus*! But at Skhul human remains were undoubtedly *H.s.s.*, as were those at Qafzeh. So in the Tabun "tradition" we find both *Neanderthals* and *Homo sapiens sapiens*. This suggests borrowing far more than cultural continuity. Moreover in the Levant, contact with *Neanderthal* is well known archeologically (Personal communication, Ofer Bar-Yosef, several times in the past 20 years). Needless to say, Tabun Cave could be the **epicenter** of *H.s.s.* and *Neanderthal* bio-genetic exchanges, now being **found by geneticists**.

The sites mentioned above suggest that, based on archeological evidence, there is no more plausible time period than the MIS 5 Interglacial for *Homo sapiens sapiens* to disperse out-of-Africa.

While the next set of sites usually lack exact counterparts in Southwest Asia, they do serve, basically, to document the LSA base in eastern Africa. Later ones also help to establish the Late Stone Age or Upper Paleolithic (in European terms).

Dispersion Two. The great "Aurignacian" or the LSA dispersal from Africa

Roughly 50,000 years after the great initial dispersion from the Horn of Africa, during a time of glaciers in the north and dry conditions in the south, the second movement of *H.s.s.* took place. This time, however, these humans held a clear **technological advantage** over both older human residents and their cousins the Neanderthals and Denisovans. The **bow and arrow**, perhaps backed up by 'atlatls' and improved spears, conferred **hunting advantages** with conceivably added competitive benefits in **inter-tribal strife**. The archeological inferences to support this hypothesis are derived from analysis of the numerous **small stone points** found widely in LSA sites, particularly in southern Africa. (See McBrearty, 2012).

Variously called **'microliths'** or **'bladelets'** or '**flakes**' or even **'blades'** these small stones formed the points of arrows, the part meant to penetrate the hide of their prey. Those composed of **obsidian** could be razor sharp and quite **easily obtained** given the presence of numerous old **vocanic** eruptions in Ethiopia and East Africa where they are usually found. In Ethiopia where archeological digs are far less common than in southern Africa, and *far far* less common than in Europe, obsidian flakes are so common on the surface that local people have a name for them, either [**balč'i**] (*Oromo*) or [**balč'ut**] (*Amhari*c).

We cannot as yet reconstruct the **genius of an invention** which knows the strength of a bent pole or stick combined with a string under tension to **hurl a stick** with a **sharp point** on it at other objects or animals. But we can infer its probable age from its presence as the **weapon of choice** even today among **all** known African hunters, including the **Pigmies** of the Congo forest, the **Bushmen** of southern Africa, the Khoisan-speaking **Hadza** and **Sandawe** of Tanzania, the several varieties of **East African hunters** in Kenya and their **cousins** in the Horn. This is particularly striking because from a pan-African standpoint it is only the isolated hunters who prefer the bow; virtually all inter-tribal warfare was conducted by **spear and shield**.

The important discovery of **arrow poisons** additionally increased the efficacy of the arrow. It was not necessary to bring down one's prey by force of impact but simple **infection** would suffice. This required extensive **knowledge** of the botanical environment and the slow, often dangerous, **discovery** of poisonous plant life, probably by **women** who are usually the gatherers among African foragers.

Any date around 50 kya corresponds to the start date for *H.s.s.* out-of-Africa, advocated by Richard Klein (2008). With these 'Late MSA' and 'Early LSA' sites we get into the **second dispersion** which is explicitly associated with **Klein's hypothesis** and with

our Borean hypothesis. While we **agree** with Klein's **dates and sites** associated with this dispersion –his **'Aurignacian'**—we hear from non-quotable sources that Klein himself is less sure of his hypothesis these days. Be that 'rumour' true or untrue, we believe that the LSA traveled from northeast Africa to southwest Asia in the time frame that Klein proposed for his 'Aurignacian'. While we will present below the Borean hypothesis as a firm support for the 'Aurignacian', here we present three supporting type sites. Klein (1999, p.401) lists and locates many more than this.

1. **Enkapune ya Muto** (GtJi12), near Lake Naivasha, Kenya, Level RBL4**; Endingi industry**, backed geometric microliths >**50 kya** (14C) > **41 ka**; Level GG/GL (OL) **Nasampolai industry**, (ObsHyd) **46 kya**; thus between **40 and ~50 kya**; DBL1**; Sakutiek industry** (14C) **35.8**; on eggshell **39.9 ±1.6**; probably OIS3 (McBrearty & Brooks, 2000; Ambrose, 1998, 2002)
2. **Mumba Shelter,** Tanzania, **Level V** (Y-series, AAR) **45-65** kya: Late MSA "Mumba industry" crescents, geometrics, backed knives; comparable to Howiespoort (South Africa). Mumba level V is 60,000 years after Mumba level VI-B. It is probably part of the "Aurignacian" or "Borean" period or Upper Paleolithic (in European terms).
3. **Ksar Akil,** Lebanon, **Level VII-XIII (14C) 32 kya, 'Late UP** Carinated' or "Levantine Aurignacian" (obsolete term), classic blade and bladelet products, soft hammer, cobble for ochre crushing**.**

There is a dearth of sites bearing *H.s.s.* fossils before 45,000 years ago in Europe, Central Asia, northern China-Mongolia, or Siberia or throughout the Americas. One investigator who has looked into the same problem supports this statement. See Oppenheimer (2009) who anticipated parts of our hypothesis. He proposed that 'anatomically modern humans' (AMH) reached the Levant around **120 kya** but "failed to continue to Europe" because of *Neanderthals*. Then "later" **(85 kya)** AMH took the "southern route" to India, via south Arabia, inhabiting mainland and insular Southeast Asia around **70 kya**, or at least before the **great Toba** volcanic eruptions (YTT), at 74 kya, then reaching Australia between **48 and 60** (or **65 kya** by bio-genetic calculations), **eastern Papua** (Huon Peninsula) by **40 kya** and **Melanesia (Bismarck Archipelago)** *circa* **40 kya**. We would add sites **Malakunanja** in far northern Australia at **52 kya** and **Lake Mungo** in New South Wales, Australia, with AMH burials at **43-45 kya**. Since the earliest known dates for Australia and eastern New Guinea differ by 20,000 years (60 kya versus 40 kya), we may have some empirical indication of the speed with which *H.s.s.* advanced from region to region. Naturally, it would only take one aberrant archeological date to change the whole equation! However, some confirmation is provided by the archeological date for **Manus Island** in the Admiralties of 20 kya. **Manus** is about 250 ocean miles (402 km) from **Papua**.

The above dates, despite appearances, are associated with the **first dispersion only**. There is no evidence that the second dispersion or the Borean language phylum intruded upon these tropical areas. Only much later would the Austric expansion and the Sinitic Chinese have clear measurable effects, primarily in establishing the Austronesian realm.

Then around 50 kya and consistent with our Borean hypothesis, Oppenheimer has AMH moving into Europe **and** Central Asia, reaching the Bering Straits between **22 and**

25 kya, crossing a land bridge, and dispersing into the Americas. His proposed movement into Europe follows a route from the Levant, through Anatolia to southeast Europe by **48 kya**.

We support this part of his hypothesis because it coincides with a probable movement linking the North African sites of **Dabba** and **Haua Fteah,** the **late MSA of the Nile Valley, Boker Tachtit** in Israel**,** the Turkish site of **Üçağızlı** in South Central Anatolia, and **Kostenki** on the Don in Europe. There is an intriguing, if not remarkable, link from Kostenki to Kara-Tenesh (layer 3) and Denisova Cave (layers 13-18) in the Altai. One possible sub-taxon of *Borean* which links *Basque* to *Caucasic* and *Burushaski* also connects them to *Yeniseian*, which is closer to the Altai. One mtDNA-based bio-genetic study, looking into Basque prehistory, connected the Basques to "Western Asia" (González, *et al*., 2006). They concluded that "It has been demonstrated, for the first time, that Basques show the oldest lineages in Europe for subhaplogroup U8a. Coalescence times for these lineages suggest their presence in the Basque country since the Upper Paleolithic."

More importantly, the roots of his Upper Paleolithic movements into Europe and Central Asia are **not** in Africa, as are ours and Klein's, but rather in southern Asia. His movement travels from India to the Levant via (roughly) the 'fertile crescent'. Oppenheimer may be correct in postulating a Southeast Asian nesting area but that proposal cannot handle the Borean hypothesis which is anchored in the Horn of Africa and has strong representation in Europe, the Levant, and Caucasus (e.g., *Basque, Sumerian, Elamitic, Kartvelian,* and *Caucasic*). Moreover, his Southeast Asian nesting area has also to account for his Upper Paleolithic expansion from both South Asia and northeast China into Central Asia, where we have only the first. Granted that we both see blockage to northern movements during much of this long period due to *Neanderthals* presumably, our hypothesis and Klein's furnish the means (i.e., Upper Paleolithic technology, especially the **bow and arrow**) for breaking out of the nesting area toward the north (McBrearty, 2012; Brown, *et al.,* 2012). Oppenheimer's excellent model lacks that impetus. But **see below** the discussion of Oppenheimer's most **recent** proposals.

With respect to the dearth of sites north of India we need to stress that this is an archeological observation. A number of bio-genetic studies have postulated a few movements to the north, based on bio-genetic inferences. We hesitate to repeat the old bromide that the absence of evidence is not evidence of absence but it is still true.

What might have a bearing on these issues would be more site data from Southeast Asia and especially Indonesia. In addition, there are Malayan sites which do not yet testify clearly in terms of dates and tool kits. Some site names are **Bukit Jawa, Kampung, Temelong,** and **Lawin.** As summed up in Oppenheimer's more recent publication, sites in the Philippines and Indonesia (Oppenheimer, 2012) are all post-Toba, i.e., **74 kya**, with the possible exception of **Punung Cave**'s tooth in Luzon. **Callao** on Luzon is close at **70 kya**. **Tabon Cave** in **Palawan,** Philippines has an upper date of **47 kya** and sure AMH fossils. Farther east in Melanesia, Papua, and Australia sites cluster in the **40 ky to 60 ky** range, while bio-genetic studies indicate a deeper genetic connection among Australians, Papuans, and Melanesians than they have with Southeast Asians (Oppenheimer, 2012). Recall Cavalli-Sforza and colleagues' (1994) 'Mainland' versus 'Insular' Southeastasians.

As a final comment on our overall considerations, we quote from remarks made about Asian archeology by John Shea (*Science News*, August, 2012, p.26).

> Some researchers argue that the meeting of biologically-related species in ancient East Asia was also a meeting of equal minds. Implements created by both species range from simple flakes struck off stones to finely chiseled blades … *Homo* populations apparently adapted tool making to environmental conditions rather than crafting increasingly complex tools over time. Contrary to conventional archeological thinking, no tool style distinguishes *Neanderthal* from *H. sapiens.*

His scenario suggests that sophisticated thinking needed for manufacturing diverse tool kits emerged 200,000 year ago or more in both species [which we call sub-species]. Tentatively we can suggest that archeological data seem to confirm a closer, if not very old, relationship between *H.s.s.* fossils and MSA tool kits in the cooler, higher region of eastern Africa from the Horn to the Cape of Good Hope. We owe part of this suggestion to Alison Brooks (2006) and John Shea (2009). However, since that highland region is closely correlated with the earliest *H.s.s.* fossils, we may be looking at shared retentions from the seminal period.

Historical Linguistics: Language, the third leg of our hypothesis, is very clear in its contribution and undeniably very controversial. Reconstructing prehistory appears, fundamentally, to depend on human bodies, material culture, and languages, although languages only become relevant after about 200 kya. Archeology and paleoanthropology were the most handicapped of our four disciplines because of their sampling problems, except for the study of societies which had writing systems, invented 6000 years ago in the Near East. We give one example of this problem. Some years ago one of us was confronted with the possible presence of cattle in the Sahara. Were they recently introduced or had there been cattle in earlier times in the Sahara? The archeology emphatically said NO! So we concluded that there had been no Saharan cattle, even though other evidence suggested that there had been. Several years later, however, an archeologist found cattle dated to the 5th millennium BC in the Sahara. Despite sampling problems, rich and well-excavated sites remain one of the most valuable things in prehistoric research. And rich areas where the sampling problem was trivial because of dense settlement, like the Nile Valley and lower Mesopotamia, became hugely informative.

In addition linguistic genetic taxonomy is capable of precise classification of every language in an area along with possible ideas on interactions between or among the languages (i.e., borrowing), reasonable clues to homelands, clear indications of time depth up to a point, and indications of outside affiliations. For example, the importance of Indo-European studies with their clear and mostly stable internal taxonomy, probable home land and time depth mostly agreed on is recognized fully by both archeology and bio-genetics.

Thus, the third source of our evidence is the field of historical linguistics, especially a modern version of it called "long range comparison." Clearly, this field has been missing in most scientific discussions of *H.s.s.* evolution and various possible pathways from Africa to other parts of the world. This was partly due to the explosion of bio-genetic research and the value of bio-genetic hypotheses; but partly due to conservative reactions in linguistics which inhibited deeper taxonomic and historical work. Yet linguistics remains very valuable as a tool for prehistoric research, even though its chronologies often leave much to be desired. Thus, while linguistics and bio-genetics frequently support each other taxonomically, they both prosper from working with archeology and its better dates.

We propose an **ancestral human** language, hereinafter **proto-Human**, which underlies **all** languages spoken by **modern** humans. It is associated with *H.s.s.* of **150 ± kya**, and was located primarily within the Horn of Africa, and branches or dialects of it accompanied *H.s.s.,* dispersing to tropical Eurasia and Australia –and throughout Africa. We also note that the proto-language of the homeland **(8)** would differ from dispersion to dispersion. Thinking about how this might be conceptualized is much easier if we consider one ancient example of such a progression: *Old Egyptian* of 3300 BC evolving into *Middle Egyptian* of 2200 BC, evolving into *New Egyptian* of 1200 BC, and terminating in *Coptic* dialects of the first millennium AD. A clear genetic language progression of three millennia, dominating a river valley for 640 kilometers and a delta about 200 kilometers wide. There may have been other languages in Egypt at various times (e.g., *Meroitic* in the far south, *Canaanite* in the far northeast, *Hyksos* or *Persian* invaders at times, or *Berber* in the far northwest, *etc.*) but no one ended *Egyptian* hegemony until *Arabic* took over.

We do not deny other linguistic possibilities, such as earlier languages evolving or being invented independently of proto-human. We deny no *Neanderthal* speech, nor do we hold any position in the rich speculation on the hardware of language origins, including human neurology, functional brain areas, hyoid bones, mouth shapes, etc. We regard it as possible that several languages may have been invented or developed by various of our ancestors and relatives, since the development of genus *Homo*. We think it possible that large regions of human speech disappeared at various times in the past. Unknown and undescribed, they are lost to our data base forever. Such losses have been proposed for parts of South America and southern Australia (Swadesh, 1960), but the clearest case of all would be the original speech of the African pigmies. **(9)** However, no one has been able to propose a plausible companion to proto-Human. We know of theoretical possibilities but proto-Human and her 5000 to 6000 daughters are all that we can actually identify.

One often hears that few linguists doubt that all known human languages have a common ancestor, although conservative historical linguists deny that such can be demonstrated empirically.

From the first contacts of Spaniards with Native Americans in the 1500s or the experiences of Dutch explorers in the 1600s (Australia) and Portuguese in the 1500s (New Guinea), no doubts are reported about the humanness of the languages or the people, notwithstanding perceptions of aboriginal "strangeness." For that reason, as well as the testimony of modern linguists, the languages of Australia and New Guinea can be assigned membership in the human language family, with the explicit presumption of descent from a common ancestor. In addition, some attempts have been made to show empirically that there is an overall human language taxon which we can call proto-Human. **(10)**

The concept of "universal grammar" or UG – by which theoretical linguists mean a basic underlying management for all human grammars – explicitly includes Australian and Papuan languages too (Noam Chomsky, personal communication, March 5, 2011). Whatever the shortcomings of that theory (UG), the inclusion of these languages in universal theory testifies to their incorporation in the human family of languages. There is thus no need to try to demonstrate empirically that Australian and Papuan languages are part of the human language family. We assume that as a given, as something agreed upon by linguists. **(11)**

Unless an intervening ancestor can be found, the implication is that *Papuan* and *Australian* languages are derived from proto-Human. The same logic would apply to the ***Austric*** super-phylum of languages, as well as scattered remnants in insular Southeast Asia, like *Andamanese, Timor, Ternate (Halmahera)*, etc. Thus, we may have four major stocks to relate to each other, viz., *Austric, Paleo-Sundic, Papuan,* and *Australian.* Sundaland is completely dominated by *Austronesian,* which is a branch of *Austric*, otherwise dominant in Southeast Asia. *Austric* also includes *Nihali* of central India and possibly *Ainu* of Japan. *Paleo-Sundic* is much broken up but still includes *Kusunda* of Nepal, *Andamanese*, and what might be called *western Papuan* (from Timor to Bird's Head in New Guinea). *Papuan* is most of New Guinea and much of Melanesia and probably Tasmania, while *Australian* is Australia. Usher (2008) tentatively proposes that *Australian* is probably related to the *Trans New Guinea* segment of *Papuan*. We choose not to follow that suggestion for the nonce.

Our reason for **stressing** the languages of tropical Asia and Australasia is to **emphasize their importance** to global level taxonomy, to point out their **large numbers**, and to remind those with western Eurasian and African orientations that the tropical cousins are the **least studied** of all the world's tongues. We probably know **less** about their **prehistory**, their **sub-classifications**, and **reconstructions**, than we know about any comparable large region in the world, **even including South America**.

The original human emigration that carried some proto-Human speakers north from Eritrea or the Sudan into the Near East, continued thence into southern Asia and thence into Sundaland and the Sahul. Because of the resulting antiquity and the consequent erosion of evidence, the linguistic phyla of Southeast Asia, Sundaland, New Guinea, and Australia have been resistant to classification, even to "long rangers."

Joseph Greenberg grouped what we call *Paleo-Sundic* and *Papuan* into one large phylum or super-phylum called *Indo-Pacific*; which he thought might later be seen to include *Australian* (see Grace, 1968). This classification was achieved before more recent data on *Kusunda, Nihali*, and many of the *Papuan* languages were published. Some British and Australian linguists criticized Greenberg's hypothesis, but no serious efforts were made to falsify it. Usher's recent appraisal (2006) neither rejects entirely nor supports Greenberg's hypothesis but rather examines the various segments of the *Indo-Pacific* realm which we pragmatically divide into two parts, *Paleo-Sundic* and *Papuan.*

This old tropical region from Bengal to Tasmania contains around 2000 languages, showing that *circa* 40% of 5000 human languages are found there or 33% if the total be 6000. Outside of *Afroasiatic* there are another 1200 or so languages which never left Africa. Thus two tropical areas of the globe are home to between 53% and 64% of human languages. This observation is very much akin to the biological one that tropical oceans contain many more species but northern oceans have fewer species with much larger populations.

Given the general but unquantified relationship between language change and time, linguists usually see linguistic diversity as an indication of time depth of occupancy in an area. A diversity of dialects means 'relatively recent', while diversity of phyla or families means much greater time depth. Thus, Italian dialects reflect a shorter time depth when compared with India with its six different phyla. The **Austric** super-phylum has been somewhat controversial because great time depths may have eroded evidence of relationship; it is composed of four phyla and concentrated mainly in Southeast Asia with one branch (*Austronesian*) spread clear across the Pacific to Hawaii and the Indian Ocean to Madagascar. Two of Austric's four phyla, **Daic** and **Miao-Yao**, are heavily concentrated in the China-Viet Nam border areas, while the other two phyla are well represented in nearby areas. **(12)**

What basis could there be to deny that some aboriginal language in Oceania, for example, was descended from proto-Human? Once X has been called a language by some human observer, what else could it be than a human natural language? Despite the ease with which journalists and the general public move "language" around from meaning to meaning, linguists are usually quite certain what a human natural language is – with considerable precision. It is a form of speaking, with a clearly defined phonological system, a grammar (morphology and syntax) which governs the arrangements possible for the core elements, and a collection usually in the thousands of morphemes or morphemic combinations (words and grammemes) which carry or encapsulate most of the semantic freight of human conversation. While any language is subject to change for numerous reasons, most of its content is passed on intact from generation to generation, such that grandfathers can converse with little difficulty with grandchildren. Languages are historical products; they are inherited, maintained or altered, and passed on to new generations. Here the analogy with biological populations and genes is striking.

The major problem with linguistic genetic taxonomy is dating or time depth. The deep chronology of human language occupation of Southeast Asia, New Guinea, and Australia, for example, is well beyond the capacity of any **glottochronological** system known so far because only **binary** comparisons are used. With a one percent retention on a Swadesh list (between two languages), one cannot exceed a time depth of 35,000 years or 18 ky to 35 ky as a range. The central value of 26 ky is the most likely. **(13)**

Each of the African phyla "max out" at the one percent (1%) level, meaning that we could not calculate the time of some ancestor linking **two** of them (e.g., *Afrasian* and *Khoisan*). There is hope that glottochronology may be extended by using large numbers of languages, 10 languages or 20 languages might yield many more millennia. But for the moment, the great super-phyla from India to Tasmania seem undatable by glottochronology but are probably much older than 26 ky or even 35 ky, if linked to each other (e.g., *Austric* and *Papuan*). The **human occupation** of the region clearly must be much **older than 50 or 60 ky**. Indeed, this basic conclusion is reinforced by Petraglia's recent findings of archeological sites in India which purportedly predate the Toba volcanic eruptions in Sumatra of 74,000 BP. **(14)**

Even between the twigs of the great *Austric* tree quite respectable time exists among closely related languages in one small cluster of Borneoan languages. *Maanyan* and *Malagasy* of the *Barito* cluster of the *Borneo* branch of the *Western Malayo-Polynesian* branch of *Austronesian* sub-phylum are said to be ~**1960 years** apart (Dyen, 1953), an estimate which is coincident with historical data on the migration of the *Malagasy* from Borneo to Madagascar (Murdoch, 1959, p.209).

Yet dating back to Wilhelm Schmidt in 1926 the taxonomy of Austric has remained controversial. Most are agreed that three primary groups of languages are involved, viz., *Miao-Yao, Austroasiatic (Mon Khmer* and *Munda),* and *Austro-Tai (Daic* and the huge *Austronesian* family, will most likely turn out to be from southeast China, ultimately). The reluctance of some good scholars to accept the whole *Austric* package is probably an argument for that super-phylum's **antiquity** in Southeast Asia. It reminds one of *Niger-Congo* which is nearly 600 languages spread over one compact area with another 500 spread over a huge land area, the southern third of Africa. No doubt, if that great African phylum were spread over hundreds of islands from Shanghai to Fiji, it too would still be controversial!

A rather similar situation appears vis-à-vis *Australian.* It has 15 sub-phyla, by the reckoning of Ruhlen's (1991) *Guide*. Fourteen of them are confined to a relatively small region in northern Australia (roughly Arnhemland and Kimberly Plateau), while one sub-phylum, *Pama-Nyungan*, occupies all the rest of Australia. There can hardly be any doubt that the north is where *proto-Australian* entered the continent and it is tempting to equate the date of proto-Australian with the archeological dates of northern Australia, especially **Malakunanja** in far northern Australia at **52 kya.**

How are we to assign time depths to the great clusters of phyla in these Oceanic and south Eurasian regions when their depths so obviously exceed any glottochronological calculations? Recent papers employing phylogenetic systematic techniques from computational biology have tried to fill the void. See Gray, *et al.,* (2009) and Bouckaert, *et al.,* (2012). Since initial testing of these methods has sometimes turned up indecisive results, we deem the approaches as promising but hesitate to rely on them. As archeology and linguistics have done in the past, we can resort to **relative dating**; something is older or younger than something else with a known or probable date or at least time period. But to begin with, since we have two or three clusters, we try to see which of them is older or younger than the others.

On the question of which language stock is correlated with which dispersion we offer a tentative answer, following the logic of the relative dating. The two *Papuan* stocks and *Australian* are probably the older because of their **locations**, greater **cladistic diversity**, ostensibly **greater time depth in India** (witness *Kusunda* in Nepal), and some tendency to appear **stratigraphically bottommost** in its area (witness *Kusunda* again, also *Timor, Alor*, *Pantar,* and *Halmahera* in Indonesia plus 23 languages in Melanesia). Yet the two *Papuan* stocks and *Australian* **lack such features in relation to each other**. Except for its vast *Austronesian* branch, whose genetic coherence has always seemed obvious to linguists, *Austric* has no representation outside of Southeast Asia and India. In the Bay of Bengal where *Andamanese* and *Nicobarese* occupy separate islands, the *Nicobarese* group, including *Shompen*, has numerous *Austric* relatives on the mainland (see van Driem, 2008), while many linguists are not even certain that *Andamanese* has any relatives at all, and even the relationship between *North* and *South Andamanese* is not easily granted. *Austric* therefore probably associates with a later phase of the **first dispersion**, circa 100 kya, while ***Paleo-Sundic, Papuan,*** and ***Australian*** **(PSPA)** probably derive from an earlier phase, circa 125 kya. Given the substantial time gap between these initial dates and the oldest archeological date on Australia, *circa* 60 kya, we can only assume that PSPA had a long term nesting area in India or Southeast Asia before moving toward Australia, possibly under pressure from the early *Austric* arrivals. Conversely, the presence of PSPA in Indonesia possibly impeded the expansion of early *Austric* into the Pacific. Oppenheimer, (2009) also proposed a similar nesting area.

The Borean Hypothesis

Following the example of Joseph Greenberg and specifically oriented towards the concept of "valid taxon" which underlies his work on Indo-European and its closest relatives, we focus on the ***Afrasian*** (Greenberg's *Afroasiatic,* formerly 'Hamito-Semitic') phylum of languages **(15)** as the western anchor of a great chain of languages extending across Eurasia and down to Tierra del Fuego in the Americas. The basic hypothesis is that ***Afrasian*** is related to the following groups of languages before it is related to any others, as they too are so related. **(16)** These kindred languages or phyla are as follows:

a) ***Kartvelian*** of the Caucasus;
b) ***Dravidian*** of greater India;
c) ***Sumerian, Elamitic*** and a few other fossil languages of the ancient Near East;
d) ***Eurasiatic*** of Greenberg, beginning with ***Etruscan*** in the far west and ending with ***Eskimo-Aleut*** in the far east. It also includes ***Indo-European, Uralic, Altaic, Korean, Japanese, Gilyak,*** and ***Chukotian***. With (a), (b), and ***Afrasian*,** it equals **"*Nostratic*"** according to some linguists; however, see below.
e) ***Vasco-Caucasic*,** proposed by Bengtson (***Basque*** of Iberia, ***Caucasic*** of the Caucasus);
f) ***Burushaski*** of Pakistan and ***Yeniseian*** of west Siberia (*Ket, Kott, Assan, Pumpokol*);
g) ***Tibeto-Burman*** *(Sino-Tibetan*) of eastern Asia;
h) ***Na-Dene*** of western North America (*Haida, Tlingit, Eyak* and 31 *Athapascan* languages);
i) ***Amerind*** outlined by Greenberg (a valid taxon with large contrasts among sub-taxa).

Although this list is noncommittal about linkages within the whole, some clusters have been strongly suggested in the literature. Except for *Afrasian* and *Amerind* which are kept distinct as anchor groups, the following clumps or clusters have been proposed. Groups (a), (b), and (d) are very convincingly combined as "*Nostratic,*" beginning with Holger Pedersen (1931), followed by Illič-Svityč (1965), Aharon Dolgopolsky (1998), and

Allan Bomhard (2006). The evidence as presented recently by Dolgopolsky and Bomhard is probably as thorough and complete as any ever presented to justify a linguistic taxon, with the possible exception of *Indo-European* itself. Differences from our presentation exist; for example, Bomhard would not include *Japanese, Korean,* or *Sumerian* at this time. He would, however, change *Etruscan*'s group name to *Tyrrhenian* which would also include *Pictish*, *Rhaetic* and *Lemnian.* Most recently Sergei Jatsemirskij (2011) has proposed adding *Minoan* (of Crete) to *Tyrrhenian.* Some would also link *Elamitic* to (b) *Dravidian* as *Elamo-Dravidian.*

More recent than Nostratic hypotheses are the proposals that groups (e) through (h) also form a significant cluster, sometimes to be called *Vasco-Dene* or *Dene-Caucasic*. The work of Sergei Starostin, Sergei Nikolayev, and John Bengtson has been most important in stitching this group together. **(17)**

The *Vasco-Dene* cluster is perhaps most notable because of the wide range of the phonological systems in different groups; *Tibeto-Burman* and *Na-Dene*, for example, are very contrastive phonetically. *Na-Dene* may be closer to *Tibeto-Burman (Sino-Tibetan)* than to other sub-taxa. *Borean* has clear similarities to M. Swadesh's *Vasco-Dene*, never fully published because of his untimely death. Modern research agrees with the *Sino-Dene* hypothesis proposed by Edward Sapir 94 years ago! ***Ainu*** is still controversial, being classified as a branch of *Eurasiatic* by some and as a branch of *Austric* by others. Japanese ethnologists have correspondingly proposed dual influences, one from the northwest and the other from the tropics; these are reflected in the Yayoi and Jomon archeological cultures, backed up by dental research. (See Hanihara, 1992). *Chinese* and its kin have phonemic tones and lack the striking glottalized consonants of *Na-Dene*. The same is true for their more remote relatives in the Caucasus with glottalics and pharyngeals. Yet dissimilarity and similarity in phonology are **typological** traits and need not necessarily reflect **genetic** relationships. **(18)**

It is clear that the Borean hypothesis involves a super-phylum some of whose sub-taxa are themselves super-phyla. The term "**phyletic chain**" is introduced as a possible label, because the Borean groups show a chain-like distribution from southern Ethiopia through southwestern Eurasia to northeast Asia and down to the southern tip of the New World. Borean is predominantly associated with human populations of "Caucasoid" or "Mongoloid" physical appearance (Cavalli-Sforza, *et al.,'s* 1994 Northeurasians), the major exceptions being southern India, southern China, southwestern Ethiopia, northern Nigeria, and the Chad Republic. *Borean* as a chain is closely associated with the appearance of the **Late Stone Age in Africa** and the **Upper Paleolithic** in the Levant, Europe, and western Eurasia from 50,000 to 35,000 years ago.

The key problem with Borean is the validity of the taxon. Do these languages show kinship with each other before they do to outsiders, such as the four super-phyla of tropical Eurasia and New Guinea-Australia and the other African super-phyla (*Niger-Congo, Nilo-Saharan,* and *Khoisan*)? For example, Greenberg saw empirical links between *Dravidian* and *Nilo-Saharan*. Trombetti found links between *Dravidian* and *Australian*. Do those links **exceed** what can be found between *Dravidian*, for example, and *Na-Dene*? And of course, how many of such links can reasonably be attributed to **borrowing**, linguistic gene flow? Given the time depths between specific phyla, hence the relative scarcity of retrievable cognates, such linkages may be difficult to obtain.

Since we have proposed that Borean is a very long chain of phyla from Ethiopia to the bottom of South America, one may ask what evidence there is for **direction of movement**. Did it originate in Tierra del Fuego, or more likely Mexico, or did it branch out from its middle, roughly Kazakhstan? One scholar (Oppenheimer, 2012) has proposed the Near East as the post-African or secondary 'homeland', influenced no doubt by the wealth of ancient written languages in that area, e.g., *Sumerian, Akkadian, Elamitic, Hurrian, Hittite,* etc., with *Egyptian* nearby. Drawing upon the strengths of 'dispersal theory' (Diebold, 1960; Dyen, 1956) and a consensus among Africanist scholars, we conclude that Ethiopia is clearly the homeland of *Afrasian* and that direction of dispersal or movement has been **outward** from Ethiopia. Despite a long tradition of **Asiatic Semitic** dominance in language and 'civilization' – long represented by the 'conquering Caucasoid cattle men' or 'Hamites' (see Greenberg, 1963) – we think that tradition was **mistaken**. *Semitic, Egyptian,* and *Berber* **came from Ethiopia.** This is the clearest indication of direction along the whole Borean chain and is quite consistent with the known movement of LSA from northeast Africa to southwest Asia and beyond. No doubt the second clearest indication of movement within *Borean*, except for *Na-Dene*, is the virtually certain movement of *Etruscan* and *Indo-European* into Europe from an *Eurasiatic* dispersal area in Kazakhstan or generally Central Asia.

Evidence for the Borean hypothesis has been presented (Fleming, 1991, 2002) but a more massive effort is now underway. Historical linguists have characteristically been very slow and very careful before accepting such large hypotheses because they are fundamentally empirical questions, not demonstrable in theory, nor to be rejected that way either.

Summation

We have argued for chronologies and a logic of association that is very unusual in modern anthropology and related fields like phylogenetics in biology, paleobotany, or Pleistocene Geology. Because there are problems virtually unique to historical linguistics, we must discuss our theses at a basic level. To accomplish this goal, we pay attention to three foci of discussion.

- **Chronology** (One of two key problems in historical linguistics)
- **The logic of associations** (Linking Borean and the Upper Paleolithic, as a kind of 'epistemic correlation')
- **Taxonomy:** Long range taxonomies.

Chronology: It is not hard to imagine the condition of **modern archeology without** its several fine dating systems. Think back only to the mid-20th century before carbon^{14} and other systems of dating became routine. Much the same is true of paleoanthropology. Remember the puzzling Near Eastern sites of **Qafzeh, Skhul**, and others before **Bar-Yosef** re-dated them to more than **90,000 BP**. Abruptly we had a date for *Homo sapiens sapiens* outside of Africa, contemporary with *Neanderthal*, and associated with the Middle Paleolithic. Archeologists have worked long and hard to build up a system of **reliable chronology** which is vital to archeological hypotheses about the prehistory of humankind. Their dating system is the **envy** of linguistics and ethnology. And while the realm of **proper written history** extends as far back as the **5th millennium BC**, it is irrelevant to much of Eurasia, most of Africa, Southeast Asia and the Pacific, and the entire New World **before 1492 AD**.

Historical linguistics has **not labored** to generate a dating system comparable to that of archeology. While a kind of **probable inference** endeavor has been recommended by some, based on reconstruction of ancestral words relating to flora, fauna, and climate, it is more useful in **proposing homelands** than dates. The well-known dating system invented by Morris Swadesh, usually called *glottochronology*, has been criticized since its inception. Linguists have seemed more interested in **refuting Swadesh's** system than in improving it. In a striking contrast, in the same half century, **archeology achieved** an excellent dating system through **tweaking** the original hypotheses, while linguistics did not tweak Swadesh's glottochronology into **usefulness**. It also seems that **mathematics** was used more to attack glottochronology than to improve the system.

Nowadays linguistics has **no viable chronology**, and no generally agreed upon way of dating proposed ancestral homelands or the frequent expansion events common to linguistic prehistory, like that of the Indo-Europeans, the Bantu, or the vast dispersion of *Austronesian* from Madagascar to Hawaii. Not to be blocked by the failures of linguistics, prehistorians now commonly attach their linguistic reconstructions to the solid conclusions of archeology or to the less reliable chronological proposals of bio-genetics. We propose that bio-genetic dates generally err on the low side of 'true' or 'real' dates, although we cannot prove this point. However that state of affairs may be changing because of new mutation rate estimates in bio-genetics. **(19)** It is surely symptomatic that Joseph Greenberg, master classifier of the 20th century and one encouraging prehistory, did **not** publish anything on **glottochronology** until 1987 when he suggested some improvements. This undoubtedly left him with no alternative, other than the accepted Clovis archeological dates, for **dating the advent of Amerind** in North America. Those dates are now believed to be underestimates for the antiquity of the earliest human entry into the Americas by both geneticists and many archeologists.

The Logic of Associations

When we make several linguistic hypotheses in our paper, such as *proto-Human* in Ethiopia or *Australian* and *Papuan* deriving from the **first** dispersion *circa* 125,000 BP, we are making probability statements of the form "it is more likely that .. .." These are **judgments of relationships, rather than mathematical probabilities**. They might take the form of "My husband is probably still working." Or "The American people will probably not bring back 'prohibition'." Or "That rope will surely break if you swing on it!" These are conclusions, if not predictions, based on someone's **assessment** of the **evidence** in any particular case. These statements are more familiar in a different form, such as

"George Washington did more for his country than any president who followed him." Or "It wasn't his money or his looks that made John Wayne so famous; 'twas his horses."

These statements are basic to **hypothesis formation**, as conceived by philosophers of science. They sum up the evidence, the facts, concerning something and propose the likely judgment. They can be **tested empirically** and judged true or false. There is **no** necessary involvement of **mathematics**. Will the rope break if you swing on it? You test that hypothesis by swinging on the rope!

This in truth leads to a strange kind of "epistemic correlation" (Northrop, 1947) – one between a hopeful but undated linguistic proposal and an accurate, definite archeological site or culture. While such correlations abound in modern prehistorical studies, they are in principle very wobbly! Yet so pessimistic are most historical linguists about dating that they may prefer the security of archeology to linguistic dates.

We argue that a **concatenation of factors** (or Whewell's consilience of induction) makes a particular hypothesis most likely. In the case of Ethiopia as homeland for *proto-Human*, evidence of **archeology**, **paleoanthropology**, and **geography** support it as the most likely place in Africa, with southern Africa in hot pursuit, for the **homeland of *H.s.s*.** at the time of the first dispersions. (Africa as the ultimate homeland is a consensus nowadays.) Since another consensus holds that **human natural language** is most clearly associated with *H.s.s*., the homeland of *H.s.s*. clearly can claim to be the homeland of *proto-Human*.

When we propose **two major dispersions** of humans from Africa at different time periods, we correlate them with **two distinct sets of languages**, supported strongly by **two** different clusters of **biological** humans (phenotypic and genotypic) correlated with **three general areas** and supported by specific datable archeological sites and two generally **distinguishable 'horizons'**.

Thus the first dispersion began leaving Africa circa 125,000 BP, traveled across northern Arabia and the Persian Gulf to India and Southeast Asia, encountering *Neanderthal* and *Denisovan* populations on the way, settled insular Southeast Asia, eventually reaching Australia by 60,000 BP at least and New Guinea perhaps simultaneously. Although largely replaced or absorbed by later dispersions from Africa, remnants remain in the so-called 'Australoid realm' between India and Australia; remnants not just in physique as in southern India but in language as in Kusunda of Nepal and far more plausibly in the Andaman Islands which are some 5000 km west of Darwin (Australia). The modern peoples derived from the first dispersions consist of the Kusunda (Nepal), the Andamanese, the native Australians, Papuans, some east Indonesians (e.g., Timor, Alor, Halmahera) and many Melanesians. Traditional race classifications commonly linked these peoples, usually under the rubric of 'Australoid', and even with the demise of 'race' in American physical anthropology, bio-genetic classifications still link them together. The Andamanese have also been called 'Australoid' and/or 'Negrito', but such are not supported by bio-genetic affiliations (Thangaraj, *et al.,* 2005).

Linguistic Genetic or Phylogenetic Taxonomy

A critical hypothesis of our paper is that there are **three major groups** of languages which are **correlated** with the **two dispersions out of Africa.** This proposal reaches **far beyond** the usual classifications of world languages. The **first** has no standard label and almost lacks scholarly efforts to join them. We have chosen to call this cluster *Paleo-Sundic, Papuan,* and *Australian* or *PSPA*. We do **not** treat this as a **genetic unit**, although a very few linguists have done so (e.g., Trombetti, Gatti, and Swadesh). Around **900** languages exceeds our capacity to deal with the question of their genetic unity **right now**, even though we believe that sometime soon their genetic unity will be proposed in a more robust manner. If *Australian* and *Papuan* have a common ancestor, it most likely lies more than 60,000 years behind them. If *Andamanese* connects up more with *Paleo-Sundic* and *Papuan*, as seems likely, then this suggests that these two, *aka* **Indo-Pacific**, separated from *Australian* much earlier than that.

The first dispersion also associates with a known super-phylum, *Austric*. Although this huge super-phylum of 1100+ languages is somewhat controversial, this is due to the **depth of diversity** in mainland Southeast Asia more than the number of **languages**. The great mass of Austronesian, circa **960 languages**, has been accepted generally for several generations and the reconstruction of proto-Austronesian is well advanced (e.g., Blust, 2009). Linking small phyla like *Daic, Miao-Yao,* or *Austro-Asiatic* to *Austronesian* has been a sticking point for some linguists but most 'long rangers' (in both the USA and Russia) accept Austric, as do some in western Europe. This was not always primarily an insular super-phylum, since it is now fairly well known that Daic at least and probably Miao-Yao too, **occupied much of China**, at least up to the level of Chang (Yangtse) river, before the advance of the Sinitic Chinese moving south. So strong is the presence of Austric in Southeast Asia that it is reasonable to suppose that the **people of Zhirendong (100,000 BP** more or less) were **ancestral** to Austric. In China, Yunnan, Guizhou, and a mountain region (Mulan) are known for ethnic diversity and conservatism. Home to the *Miao* of Austric, this area is ethnically part of Southeast Asia, not Sinitic China. Most research reports distinguish between these peoples and the Han Chinese. Recently, M. Erard reported that 24 new languages had been discovered in Yunnan through field work done by J. Pelky. See Erard (2009).

Could *Austric* be related more to *Australian* and its brethren than to *Borean*, because *Austric* is closer to *Australian* in time? Russian colleagues suggest that *Austric* is related to *Borean* (see **16**). It is also striking that proposals of *Austric-Papuan* or *Austric-Australian* relationships are very rare. No doubt such exist; we just do not know of them. One caution would be that *Austric* languages have been in contact with *Borean* languages for a long time, upwards of 40,000 years in India and China, while Papuans and Australians had little Borean contact until very recently. Linguistic **contact** over millennia would ordinarily lead to **borrowing** and **influence**. The *Chinese-Daic* example of this is now famous with each side receiving a great deal from the other.

The **second major dispersion** proposes a new linguistic group, one of the largest in the world in **population sizes** and **geographical extent**. Borean is **bold but well supported**. Its 1500 languages occupy all of North and South America, Europe, half of

Africa, most of Eurasia, and almost all of Australia. They dominate the world culturally and politically, having done so for hundreds of years. Borean is a **younger** linguistic **taxon** than its counterparts in Africa and the southwest Pacific. It is thus **more likely** to have **preserved evidence** of its past than its counterparts have. It also has the **advantage** of containing all of the **ancient** languages (e.g., *Sumerian. Egyptian, Sanskrit, Chinese, Mayan, et al.*) known to us, excepting *Meroitic*, and the bulk of the inquiry into relationships and ancestral forms. It is almost certainly true, but undocumented, that more historical linguistic work has been done on a few *Borean* languages than on **all the non-Borean languages of the world.**

Although one of us (Fleming) first proposed Borean to the world, the hypothesis is truly the outcome of fruitful work done by many people. Borean depends on the tremendous increase in research on three main foci after World War II. The **Nostratic** hypothesis, the **Dene-Caucasic** hypothesis, and the **Amerind** hypothesis – each a tremendous effort and accomplishment in itself – these are the *sine qua non* of Borean. Each was amply documented, often bitterly opposed, but also revised to suit the more cogent criticisms. Many linguists were critical of these hypotheses; in the case of Amerind the opponents might be described as angry. Many more linguists handled the problem of new taxa such as these by disregarding them. This is perhaps not what scientists are expected to do with new hypotheses.

Looking at this from the logic of science, rather than polemically or judgmentally, we may agree that hypotheses are meant to be tested. In an overwhelmingly empirical science like historical linguistics –which cannot do laboratory testing –one has to examine the evidence and determine how much of it is to be condemned, i.e., falsified. There are tried and true ways to falsify historical hypotheses in historical linguistics. Seek to destroy (falsify) the proposed cognates, **incipient etymologies,** upon which **both sound laws** and **reconstruction depend.** If one says that English 'hound', French 'chien' and Avestan 'span' are cognate, i.e., descended from a common form, then one might reject that proposal as absurd because the forms all look different.

Since few linguists produce perfect sets of proposed cognates, critics often win arguments about specific etymologies. So the key question becomes **how many** etymologies can be destroyed before the **entire hypothesis** has to be abandoned **as false?** Or how many parts of the whole have to survive for the whole to remain credible? Before we use our imaginations on that question, we find a surprise! The question was once answered publicly by Dr. Ives Goddard, as one opposing *Amerind*, at a serious meeting of opponents and proponents of Greenberg's historical methodology at Stanford University in 1986. **Goddard's answer was 35 convincing etymologies**. One of us was sitting directly behind Goddard when he made that remark. It seemed a perfectly reasonable quantity to demand. Since Greenberg mustered **281 Amerind etymologies**, not just regional ones, and we cannot believe that **247** of them were **mistaken**, we think that Amerind **passed** the Goddard test!

Thus far presentations about Borean have been largely ignored, except by 'long rangers' in Russia and the USA. Its **scope and number** of languages discourages many from attempting to evaluate it. However on a simple level we can report that Borean has already **passed** the Goddard test. As of now some **47 lexical** etymologies, as well as **27**

'grammemes' (grammatical morphemes) have been compiled. If only half of them are true, which would be a low performance record, still our **37 true** ones would suggest that when we begin our **massive comparative work**, we will pass the Goddard test and have plenty of room to spare. **Then we are off to greater Papua!**

A comment comes from Greenberg (1987, Preface). Concerning etymologies, he writes:

> Particularly in regard to etymologies, any user of existing dictionaries, even of intensely studied languages, will encounter numerous instances in which the same form has been assigned to different etymological entities by different scholars, or even in which the same form has erroneously been included in different etymologies.... Still while some etymologies are virtually certain or highly probable, others are marginal and will perhaps never be finally decided..... If the strength of Indo-European studies is largely based on the existence, in a few instances at least, of very old sources, the strength of Amerindian studies is simply the vast number of languages. The synchronic breadth becomes the source of diachronic depth.

The **hundreds** of Papuan and African languages are a basis for **hope, not despair**. We advise, lest it be forgotten, that each proposed **cognate set** is in fact a **small hypothesis**, that each **form (morpheme)** in the set is descended from a **common ancestor**.

A most recent study by Rasmussen, *et al.*, (2011) produces DNA evidence that *H.s.s.* entered eastern Asia between 62 kya and 75 kya. Their other finding was of a second "migration" around 25 kya to 38 kya. Their data were taken from a native Australian's hair (before European contact), as well as from Han Chinese, West African (Yoruba) and Europeans. We caution that these population samples may be inadequate for a global conclusion, with samples from Papua, Burma, south India, and southwest Ethiopia being desirable additions. We also wonder if their dates might turn out to be underestimates for their two migrational episodes as predicted by the proposed dates of our dispersal hypotheses.

Oppenheimer's Most Recent Proposals: A Critique

While any number of bio-genetic studies have suggested dates for *H.s.s.* emergence from Africa and specific connections to various non-African populations, there have been only five more ambitious general proposals: Richard Klein, in several publications, Alison Brooks and Sally McBrearty in two major publications, Stephen Oppenheimer in two different scenarios, plus Henn, Cavalli-Sforza, and Feldman (2011). Interestingly, only one (Klein) specified that human language accompanied *H.s.s.* out of Africa. Klein grounded his proposals in the **LSA** or **UP** of **50 kya** or later, while **denying** full *H.s.s.* or **AMH** status to earlier emergences like **Qafzeh**. His human language accompaniment was more like a **suitcase**, with no phylogeny or detail on various language phyla. Since Klein ran afoul of definitional problems of AMH and his commitment to UP dates, his proposal is probably being quietly shelved. Brooks (2006) focused on the period around 70 kya, thus breaking out of Klein's limitations. She had no language component. Oppenheimer basically had two proposals, an earlier and the most recent (2012). His first proposal was far richer in non-African information than either Klein's or Brooks'. It included a date of **120 kya** for initial out-of-Africa, explanations for the peopling of southern Eurasia and Australasia-Oceania, and a sojourn in the south before proposed movements into northern Eurasia, roughly the same as Klein's 'Aurignacian'. But there was no human language component. (More on Henn and colleagues below.)

Oppenheimer's more recent proposal (2012) also lacks any human language involvement but a deep commitment to bio-genetic findings is central to his new hypotheses. His overall hypothesis is anchored in two propositions, the first that there was **only one dispersion** from Africa and second that no serious or lasting emigrations took place before the great Toba volcano exploded circa **74 kya**. Oppenheimer runs roughshod over the archeological sites so important to earlier dates, like Abdur Reef, Jebel Faya, and both Qafzeh and Zhirendong, either denying their further expansion, i.e., genetic 'dead ends', or associating them with 'archaic' humanity, thus repeating Klein's argument and probable error, or criticizing the site report itself. By standing firmly on the validity of bio-genetic dating (molecular clock) and mtDNA phylogeny, Oppenheimer sets himself up for the falsification provided by the recent reforms in bio-genetic dating (see **19** below). That is to say, most of his genetic dates are probably many thousands of years too young or too shallow.

The arbitrariness of his dismissal of so many archeological dates is for us good reason to reject his basic hypothesis. How do you know that Jebel Faya was a genetic dead end? Our answer is that Jebel Faya represents the eastern end of a long line of sites extending back to Ethiopia. Why would one assume that this culture was terminal at Hormuz? But most of all why assume that bio-genetic data and conclusions are paramount, so that they overrule archeological results and totally ignore those very valuable linguistic conclusions?

Addenda

Part One. One recent development, too fresh to have been thoroughly evaluated, is the proposed new population of *Homo* which is said to have co-existed with *H.s.s.* in central and southern Africa, most probably in the Rift Valley lakes region of East Africa. This conjecture is based on bio-genetic data from three hunter-gatherer populations, viz. Pigmies of Cameroon plus Hadza and Sandawe of Tanzania. Data were gathered on 15 individual genomes and not limited to mtDNA or Y-chromosome data. The modern Africans sampled are said to have about 2% of their genomes of alien origin. The terminal date for contact with the new hominin group was reckoned to be from 70 kya to 30 kya, the estimated date for the separation of the three African populations. (See Lachance, J., *et al.*, 2012)

Part Two. One new and strongly supportive linguistic date can be added to our hypotheses. According to Perreault and Mathew (P&M) (2012), the original human language, or what we call proto-Human, is to be associated with the Middle Stone Age (Africa), **"sometime between 350-150 kya"** or **250 kya** on average. This is based on archeological evidence for the MSA as a whole. Using fossil evidence would give **195-160 kya** for AMH humans. Supposing that "fossils classified as *Homo helmei*, that may be anatomically modern or nearly modern, are dated to **300-250 kya"** that would extend the inquiry beyond our parameters. (We do not believe that *Homo helmei* data are sufficiently well known or analyzed to be helpful.) Our proto-Human is **defined linguistically**, as the root or ancestral language to all modern or known human languages – nothing more. P&M posit a language which would be correlated with the archeology of the African Middle Stone Age. Their proto-language is not defined linguistically. Rather it is a **theoretical** entity and need not necessarily be the same as our *proto-Human* or possibly be anywhere near it in geography or time period.

P&M's proposal is welcome and excellently presented. Since it does consist of a whole host of assumptions about phonetic change and fairly *ad hoc* assumptions about outcomes, it is difficult to use or even evaluate properly. Moreover, in at least two aspects it is empirically mistaken. They use **60 to 70 kya** as the dates for the emergence of H.s.s. from Africa. Given current information, as we have outlined before, those dates are **40 ky** to **70 ky** too young or **too shallow**, either archeologically or as calculated by recent reforms of the molecular clock. **(19)** Their ground assumption that the number of phonemes decreases with distance from the source, or what linguists would call "homelands," can be falsified several times, e.g., *Bantu* of South Africa, *Na-Dene* in relation to its obvious homeland in eastern Asia, or *Modern South Arabian* in relation to northern Semitic languages such as *Ugaritic, Hebrew, Akkadian, et al.*. Indeed, their important case of Khoisan languages contradicts their main thesis. Here a language called *!Xun* allegedly has 141 phonemes which far exceeds the counts of 62 in *Hadza* or 54 in *Sandawe* up in the *Khoisan* homeland in Tanzania – the opposite of what is predicted.

Perhaps the most surprising thing about P&M's study is that it was not written by linguists, probably, because the matter of increase or decrease in phonemes was put in **areal** terms, **not** in language taxonomy and reconstruction terms. A linguist would have compared proto-languages with daughter languages, not African languages with Asian languages. As some Africanist linguists have done, one can be sceptical of the Khoisan language with 141 phonemes. Nor would a historical linguist lump together many Southeast Asian languages to compare with Andamanese. If they are not genetically related, how can one be compared to the other in terms of source and outcome?

Part Three. Although we hesitate to present **primary data on linguistic analyses,** one Borean etymology is so striking and convincing that we have decided to include it. Joseph Greenberg (2002, p.2-3) argued briefly that Eurasiatic was clearly the closest relative of Amerind and included an etymology for **"hand, give, measure",** citing data from 17 *Eurasiatic* languages, 6 of which were proto-languages, compared with 31 *Amerind* languages, 8 of which were proto-languages. Here we wish to extend that etymology to the rest of Borean by including relevant data in the same meanings from *Afrasian* and other segments besides *Eurasiatic* and *Amerind.*

In the meaning of "give" in *Afrasian* there is: *Old Egyptian* [**1m1**]; *North Omotic*: *Kafa, Anfillo, Shinasha, Mocha* [**ˀim**], *Chara* [**im**]; *Yemsa (Janjero)* [**ima**], *Basketo* [**um**], *Male* [**iŋg**[, *Dorze, Ganjule* [**im**] and [**îŋg**], *Oyda* [**ing**], *Koyra (Koorete)* [**iŋ**]; South Omotic: *Dime* [**ˀim**], *Galila* [**ˀum**], *Hamar* [**im**]; North Cushitic: *Beja* [**hiw**]; Central Cushitic (*Agau*): *Bilen* [**iw**] and [**uw**], *Khamta/Chamir* [**iiw**] or [**iuw**]; East Cushitic: *Hadiya* [**uuw**], *Sidamo*[**uw**], *Darasa (Gedeˀo)* [**iuw**], *Burji* [**u**] or [**w**]; South Cushitic: *Mbugu (Maˀa)* [**awi**] possibly from [**qaw**] and not cognate. Or *Mbugu* [**ˀo**]: *Semitic*:

Ethiopic: *Ennemor* [**yiim**] and [**ama**], *Mesmes* [**hamo**]. It is not found in *Berber* or *Chadic*, except probably in the *South Bauchi* group of Chadic in the meaning of "hand" or "arm."

In the meaning of "hand" or "arm" or "cubit" in Afrasian there is: *Old Egyptian* [**mH**] 'cubit'; *Chadic : West Chadic: Polci* [**aam**], *Ngizim* [**amai**], *Bade* [**âemi**], *Geji* [**aŋ**], *Guruntum* [**aa**] ~ [**ŋ**]; Central Chadic: *Gisiga* [**han**] and *Semitic* 'cubit': *Old South Arabian* [**ˀmt**], plural [**ˀmm**] and [**ˀmn**], *Soqotri* [**ˀemeh**], *Hebrew* [**ˀammaa**], *Ancient Aramaic* [**ˀmh**], *Syriac* [**ˀammit̠aa**], *Ugaritic* [**amt**], *Mandaic* [**ama**], *Akkadian* [**ammatu**] and *Geez* [**ˀimat**]. *Modern Ethiopic: Tigre* [**ˀammat**], *Tigrinya* [**ˀimmat**].

In the possible *Vasco-Dene* section of *Borean,* thanks to Sergei Starostin and his colleagues we have *proto-Sino-Caucasic* [**VmVn**] for "to give" and for *proto-North Caucasic* [**mēHwV**] for "hand, extremity", where [**V**] = some vowel and [**H**] is the pharyngeal. Individual languages include *Avar* [**maHu**], *Chadakolob* [**mahu**], *Chamalal* [**maH**] and *Karata* [**nihe**] from [***mihe**]. In *Dargwa* there is *proto-Dargwa* [**meH**] "hollow of hand, handful" and *Lak* [**yama**] in [**k'wi-yama**] = "handful". In *Lezghian* the proto-language has [***hem**] or [***hema**] and *Udi* specifically has [**aim**]. In the form **of [mV]** the root is embedded in *West Caucasic* or *Abkhaz-Adyghe* languages, for example *Abkhaz* [**ma-čwa**] **= "finger".** In *West Caucasic* original [**m**] often changes to [**ṗ**] by dissimulation from [**ma-ṗa**].

In the *Basque* and *Yeniseian* 'branches' of *Vasco-Dene* which Starostin calls '*Sino-Caucasian'* we have *proto-Yeniseian* [***pVn**]. *Proto-Yeniseian* has no [***m**], except in a few expressive words, for which [**p**] is substituted. *Proto-Basque*, the ancestor of seven modern varieties or 'dialects', has [***eman**] 'to give'.

In *Burushaski, Yasin* dialect, [**maṥ**] = "stretch out hand, touch with fingers, [**d-mas**] = 'hand over', [**mać**] 'fingers, [**meṣ**] 'finger'.

We have not searched Tibeto-Burman, Na-Dene, Kartvelian, or Dravidian languages because of space limitations of this paper.

Part Four.While bio-genetic closeness or distance does not prove their linguistic genetic counterparts, the two are often highly correlated. Because of their acute oceanic separation from their Asian cousins it is often difficult to determine which phylum is closest to the isolated ***Na-Dene*** group in North America. Strong biogenetic clues, based on **recent** and **highly focused** research (Reich, *et al.,* 2012), indicate *Na-Dene* ties to ***Sino-Tibetan***, as **the closest bio-genetic kin of the Na-Dene.** Since the **primary population** of their sample was **Han Chinese**, rather than a more generalized "Chinese" with its likelihood of including absorbed elements **of *Austric* origin** from south China, the study tends to back up the linguistic hypotheses of *Vasco-Dene* which link *Sino-Tibetan* and *Na-Dene* with *Yeniseian* and the Caucasus. Modern biological research agrees with the venturesome *Sino-Dene* linguistic hypothesis advanced by Edward Sapir 94 years ago! Linguists such as Swadesh and Greenberg, among others, have also agreed with Sapir's conclusion and saluted a hypothesis willing to cross the vast distances of the Pacific Ocean to connect two very different taxa!

Part Five. Until recently, the primacy of Ethiopia or the Horn of Africa or "northeastern Africa" as the springboard for *H.s.s.* dispersions to Asia has not been seriously challenged. Two recent proposals, however, need to be discussed briefly. One (Hublin and Klein, 2012) proposes that the **Aterian** segment of MSA was a **possible**

source of modern human expansion. This is due to the fact that **"Aterian craniodental fossils resemble fossils ...at the Skhul and Qafzeh Caves in Israel."** This proposal represents a basic change in Klein's viewpoint, expressed earlier. Otherwise, we would **not deny** that the Aterian branch of MSA had a part to play in the first dispersion, as did the Ethiopian branches, e.g., Abdur Reef and Aduma and probably Nile Nubian. But Scerri (2012) also reckoned that the (Aterian) peoples were ultimately of sub-Saharan origin, or as we have proposed, they dispersed from Ethiopia by way of the Sahel and Lake Chad and the (interglacial) Saharan wet spots. The pattern is strongly reminiscent of a much later one of the pastoral Neolithic in the Saharan region.

Another hypothesis (Henn, *et al*., 2011) puts the human source population in southern Africa among the ancestors of the Bushmen hunter-gatherers. As characterized by Hublin and Klein (2012), Henn's thesis is that "...**the source population for modern humans, including the group that expanded from Africa to Eurasia roughly 60,000 years ago, resided in southern Africa**." As we have argued earlier, the cooler uplands of eastern Africa from Eritrea to South Africa shared the MSA and no specific region has as yet triumphed as **The Source area**. As argued, however, Henn, *et al.*'s (2011) **dates** are much **too recent** to claim the honor of source. Since their reasoning is fundamentally based on bio-genetics, rather than archeology plus bio-genetics, their proposed numbers such as 60,000 BP are almost certainly wrong, and on the face of it, much younger that the archeologically-based Ethiopian dates. Finally, like many studies coming out recently, their proposed hunter-gatherer populations are mostly Khoisan speakers or Pigmies; the numerous Nilo-Saharan and Afrasian-speaking hunters of Kenya, Uganda, and the Horn are **consistently ignored**. Since most of the Horn has not been sampled bio-genetically, and hunters are fairly common, it is surely premature to conclude that Kalahari hunters are the last word in bio-genetic contributions to prehistory.

End Notes

1) See Klein (1999) and for a recent re-statement of his argument see (2008). Many scholars have adopted his viewpoint on "fully modern," while many have not. The argument is basically theoretical, focused on "fully modern behavior."

2) Ethnologist Daniel McCall once noted that the modern English were markedly different in musical and artistic (painting) creativity from the French and the Germans. (See McCall, 2007). The anthropologist, Eric ten Raa, reported that (Khoisan) Sandawe hunters once walked down to Dar es Salaam, learned how to sail an Arab *dhow*, then sailed to England and back, stopping finally in Australia. Once in 19th century Australia, a group of 'aborigines' learned to play cricket. They traveled to England where they defeated the local teams (ten Raa, 1962). For two archeologists' critical view of this question, see Shea (2011) and McBrearty (2012). For the critical views of a physical anthropologist, see Landau (1984). We believe that Klein's argument about head measurements in relation to intelligence or symbolic culture would **not** survive a rigorous ethnographic appraisal cross-culturally. We note that in the modern world the music and artistic bents of Ethiopia or Somalia and those of Nigeria or Ghana differ markedly.

3) Although the debate over whether a species or a subspecies best describes either *Neanderthal* or *Homo sapiens sapiens*, or both, is not settled, we prefer to regard both as **subspecies** of *Homo sapiens*. The key evidence: inter-breeding in Eurasia.

4) For tool kit continuity in Ethiopia, see Shea, Fleagle, and Assefa (2007). For the primary site in Ethiopia see McDougall, *et al*., (2005).

5) Further mtDNA studies have **confirmed Eve's date**s at around **200 kya**. See Soares, *et al*., (2009); Behar, *et al*., (2008); Gonder, *et al*., (2007); Mishmar, *et al*., (2003). For Australia, such studies indicate a coalescence time for both N- and M- lineages of around 66 kya, presumably in the north. Then the central desert by 48 kya and New South Wales by 40 kya. See Hudjashov, *et al*., (2007). There is also fossil *H.s.s.* at Lake Mungo, New South Wales, conservatively dated around 40 to 43 kya.

6) Specific sites are Klasies River Mouth, Blombos Cave, Abdur Reef, Taramsa 1, El Guettar, Skhul-Layer B, Qafzeh, Jebel Faya, Patpara. See Klein (1999) for tooth sizes of Aterian and other sites.

7) The difference between 'dispersion' and 'migration' can be readily seen in the movements of west Europeans into North America in the late 2nd millennium AD. Five distinct movements can be seen on one horizon, the European invasion of North America. Going from the north, we have (a) the **French** following the St. Lawrence into the northern interior, (b) the **Yankee** or Puritan going north of the Appalachians towards the Great Lakes, thence to Oregon and California, (c) the **Southern** or Cavalier going inland southeast of the Appalachians, bending west into Texas and the Southwest, (d) the **Apppalachians** or **Scotch-Irish** following the Appalachian chain from Pennsylvania, ultimately to Texas, and (e) the **Spanish** or **Conquistador** movement, the earliest, moving across the Caribbean, thence to Mexico, thence north into the Southwest and south into Central and South America. This account is derived from the recognized history of European settlement in North America, combined with the same areas' known dialect geography.

Archeologically, the five movements contained highly similar but distinctive tool kits. Physically, the people were quite similar but with some distinctions, with small DNA differences. The languages were similar, indeed related, coming from one phylum, Indo-European. Within six centuries, this dispersion had occupied the whole continent, despite sometimes vigorous opposition from the autochthones, who were largely wiped out. There was some interbreeding with the natives, reaching significant percentages primarily in the Spanish realm, where the largest native populations are thought to have been. Early *H.s.s.* had 42 times as much time to occupy a much larger realm, but, of course, much less technological advantage over the autochthones, until the LSA period when the bow and arrow was added to the LSA tool kit (McBrearty, 2012).

8) We do not know exactly when language was invented or developed and we eschew all attempts to find out exactly when. We submit that such a question will have to wait upon the **reconstruction** of *proto-Human* before it can be answered. We doubt that the answers will be found in the **'hardware'.**

9) There is a clear consensus among Africanists that the original Pigmy speech was replaced by *Bantu* or *Sudanic* languages, which moved into the forests in the past 5000 years. Ten numbers of a language called *Dima*, spoken by supposed Pigmies in southern Ethiopia in the early 20th century (Conti Rossini, 1927) cannot be related to any other number set in Africa; these are the only known Pigmy language data, not created for tourists to wonder at. Further research by Bahuchet and his colleagues may be able to reconstruct some of the original Pigmy language from phonetic patterns and unusual lexical items.

10) See Bengtson and Ruhlen (1994). Support for them can be found in Pagel (2000). Very recent empirical support can be found in Matthey, *et al*., (2011) who argue that the vast global distribution of kinship terms like [**papa**], [**mama**], and [**kaka**], usually for "father", "mother" and "uncle" or "grandparent" cannot be explained by theories of baby talk or spontaneous invention through the linguistic experimentations of children trying out the easily accessible sounds like [**m**], [**b**] or [**p**].

Rather these kinship terms can be most readily explained **historically**, as **products** of inheritance from proto-Human. Especially telling is the case of [**kaka**] type words which were not usually included in theories of baby talk.

11) Some debate whether UG is "**hardwired**" genetically in the brain or whether it is universal "**software**." S. Pinker likens UG to a tool kit which a person uses to construct a grammar. However, UG has been denounced by many linguists and psycholinguists for being too nebulous, too dogmatic, and not in fact universal. See Evans and Levinson (2009), especially a contribution by M. Tomasello. The critics, nevertheless, explicitly include Australian and Papuan languages in examples of human language behavior.

12) Analogously, another indication of diversity equaling greater age appeared in the case of frogs in the Amazon. Where the argument was

"Diversity Takes Time"

> "Like many groups of organisms in the Amazonian tropical rainforest, hylid tree frogs show a very high diversity. Moreover, there is strong variation in local diversity, with some localities and regions having much higher density of species than others. Wiens, *et al*., (2011) take a phylogenetic approach to the question of the cause of this local variation. Their analysis indicates that there is little or no relationship between variation in local species richness and climate variables such as temperature and precipitation. Nor are the rates of diversification or morphological variation correlated with local richness. Instead, diversity is related to the length of time that hylids have occupied a region. Even though diversification rates slow down when multiple clades occupy a region, species nonetheless continue to accumulate with the length of time that the region has been occupied. The highest diversity occurs when the largest number of clades have coexisted for longest**."** For the tree frogs see Sugden (2011).

13) See Kruskal, *et al.,* (1973). As calculated by the most 'liberal' or the system with the oldest dates. Other systems and the standard original one yield 10 ky to 20 ky at one per cent (1%) retention. At zero percent (0%), which is reached in some African phyla, chronology becomes impossible. Glottochronology is comparable to radiocarbon dating in its chronology. A similar system was invented by the Russian linguist, Sergei Starostin, who has a shallower chronology. Taxonomically useful evidence may still be abundant at the zero to one per cent level, e.g., morphological, phonological, or core lexical evidence. Such occurs between *Omotic* and *Semitic* languages in *Afrasian* or between *West Atlantic* and *Adamawa-Eastern* in *Niger-Congo*. For example, *Semitic Geez* [**nage**] and [**noge**] = 'elephant' and [**nagot**] = 'trunk', while in *Omotic Dime* [**nuku**] = 'nose'. These cognate terms would be scored as negative on a Swadesh glottochronological list because *Geez* has a different term for 'nose', while *Dime* has a different word for 'elephant'. Or *Ancient Egyptian* has [**s-n**] for 'smell' while *Omotic Basketo* has [**sin-ts**] for 'nose'. Again, two cognate terms, but not on the glottochronological list, where the *Egyptian* word would be [**f-n-d̲**], a non-cognate word, despite much similarity of consonants. See *Semitic Amharic* [**afinč'a**] 'nose' for the cognate to the *Egyptian* form.

14) We mention one attempt at calculation in *Austric*. As reported by Isidore Dyen (1965), one of the *Austronesian* primary branches on Formosa (possibly *Paiwanic*) scored **6%** of retention compared with a *Melanesian* language of the *Oceanic* division of *Eastern Malayo-Polynesian* of the *Malayo-Polynesian* branch of **Austronesian**. These are probably maximally divergent for *Austronesian,* one of two branches of *Austro-Thai*, itself one of the primary branches of *Austric.* That is reckoned as about 9000 years by the Joos system, reported by Greenberg (1987, p.342), or from **12,250 to 17,150 (avg. < 15 kya)** by Kruskal, *et al.*'s tables (1973, p.36). The overall average would be ~ **12 kya.** And it would be a candidate for the date of *proto-Austronesian*. Then *proto-Austric* would be much older than that. Incidentally, at an equal percentage of retention these figures would be likely to date *proto-Pama-Nyungan* in Australia which came to occupy most of Australia. (Calculations by Geoffrey O'Grady.)

15) See Greenberg (1995): nearly all 70+ languages of Ethiopia, Eritrea, and Somalia belong to *Afrasian (Afroasiatic*). The few exceptions are *Nilo-Saharan* languages along the western borders with the Sudan. Not only have scholars for centuries linked Ethiopian languages to those of the Near East and Egypt, but also the Borean hypothesis postulates them as the anchor of the Borean phyletic chain. Greenberg's concept of "valid taxon" is perhaps inappropriately named. It means that a taxon is formed when all members are closer to each other than they are to others. It corresponds to the concept of "monophyletic group" in biology. Thus, *English, Dutch,* and *Swedish* form a valid taxon for this reason: three *Germanic* languages. But *English, Swedish,* and *Hindi* also form a valid taxon for the same reason but at a higher level, *Indo-European*. On the face of it *English, Swedish*, and *Hindi* do not form a valid taxon because *Hindi* is related to *Gujarati* and *Kurdish* before it is related to *Swedish* and *English*. In mammalian terms, whales, orcas, and dolphins form a valid taxon but whales, orcas, and tigers do not because tigers belong with other cats. But in relation to sharks, all three form a valid taxon as mammals.

16) See Fleming (2002). This paper was also presented as a poster at Cold Spring Harbor Laboratory in October, 2002 and in a paper in *Mother Tongue, the Newsletter* (1991). Others later proposed a *Borean* with *Austric* added to it but without *Amerind.* See Gell-Mann, Peiros, and Starostin (2009). That in turn was similar to an earlier Russian version by A.Y. Aikhenvald-Angenot and J-P. Angenot (1989, 1992), which was circulated informally but published in *Mother Tongue: The Newsletter*. The theory, called NOSCAU, portrayed *Austric* as the third of three coordinate branches of NOSCAU, thus suggesting that *Austric* shares a common ancestor with the other two branches. There is no indication that *Indo-Pacific*, for example, was compared to *Borean* in this way. The NOSCAU proposal or *Nostratic-Sino-Caucasic-Austric* presented grammatical evidence, mostly pronouns. Most noteworthy was that neither *Amerind* nor *Sumerian* were included in NOSCAU, a serious difference with our *Borean*. It is possible that the authors did not try to link *Amerind* to the rest because they had not noted the very strong pronominal evidence linking *Amerind* and *Afrasian*, for example.

17) See Starostin (1988). Some additional Russian support for the Sino-Caucasic and Borean hypotheses can be found in Orel (1995). The Abstract says: "Basing on over 150 roots supposedly common to the three languages [sic], a rather simple set of consonant correspondences can be formulated. The name Paleolithic is proposed for the hypothetical proto-language." We believe that Borean at least is to be associated with the Upper Paleolithic (of Eurasia).

18) For example, within the prevailingly "harsh" phonetics of ***Semitic*** languages there are those who altogether lack or have very few glottalics (or emphatics) or pharyngeals, e.g., *Mesmes* of Ethiopia, *Maltese Arabic*, and of course *Modern Hebrew*. In South Africa, *Zulu* and *Xhosa* with numerous clicks and glottalics contrast sharply with other *Bantu* languages: the reason is known to be close contact with *Khoisan* speakers. *Pokomo*, a *Bantu* language of eastern Kenya, also has a set of glottalic consonants, due to contact with *Cushitic* languages. In *Egyptian*, after 3000 years, there is a sharp difference between the early stages and the latest, with most of the "harsh" phones of the original being lost.

19) See P. Soares, *et al.*, (2009), reported by James Harrod (2011). The problem of bio-genetic dating is **deeper** than they thought, however. See Ann Gibbons (2012) for a summary of recent research which has **re-set the biological clock**, so to speak, such that **most mtDNA dates** are **much older** than presently hypothesized. See A. Scally and R. Durbin (2012) for a primary discussion. Under the new clock Asians and Europeans diverge ~ **50 kya**, while Yoruba and non-Africans diverge at **110 kya**. However, one of their dates is simply unbelievable, i.e., they date the split between the **Khoe** (Hottentots) and the **San** (Bushmen) at >250 kya. **No Africanist could begin to**

accept that date! It is quite possible that their sampling picked up some Berg Dama or other divergent group (See Addenda, Part One). For another example, and a more satisfying one, Gibbons (2012) changes old dates of **<70 ky** to new dates of **90-130 ky** which fits our basic hypothesis and is much more in accord with fossil and archeological dates.

Apologia

A goodly amount of data are being published, or have been published in recent years, in the blooming field of **archeogenetics**. Some of this material has been incorporated in our article. We have had to forego mentioning many studies, not crucial to our hypothesis, lest we end up with a book. Our apologies!

Bibliography

AJHG	American Journal of Human Genetics.
AJPA	American Journal of Physical Anthropology
PNAS USA	Proceedings of the National Academy of Sciences, U.S.A .

Abdulla, M.A., *et al.* 2009. Mapping human genetic diversity in Asia. *Science* 326: 1541-1545.

Aikhenvald-Angenot, A.Y. & J-P. Angenot. 1989. On Some Possible Isoglosses Between Eurasian Proto-Nostratic and South-American Proto-Je. Paper presented at the Conference Lingvističeskaja rekonstrukcija i drevnejšaja istorija Vostoka (29 maja - 2 ijunja 1989, Moskva: Institut Vostokovedenija). MS. 22 pp.

Aikhenvald-Angenot, A.Y. & J-P. Angenot. 1992. The South-American Proto-Ge and the Old World. In V. Shevoroskhin, ed., *Nostratic, Dene-Caucasian, Austric, and Amerind,* pp. 403-417. Bochum: Brockmeyer.

Ambrose, S. 1998. Chronology of the Later Stone Age and food production in east Africa. *Journal of Archeological Science* 25: 377-392.

Ambrose, S. 2002. Small things remembered: origins of early microlithic revolution in sub-Saharan Africa. *Archeol. Papers Am. Anthropl. Assoc.* 12: 9-29.

Anati, E. 2006. 1200 sites corpus. Har Karkom: 20 years of biblical archeology by Emmanuel Anati: http://www.harkom.com. **Online 2006.**

Anthony, D. W. 2007. *The Horse, the Wheel, and Language: How Bronze-Age Riders from the Eurasian Steppes Shaped the Modern World.* Princeton, N.J.: Princeton University Press.

Armitage, S.J., *et al.* 2011. The southern route 'Out of Africa': evidence for an early expansion of modern humans into Arabia. *Science* 331: 453.

Baily, G. 2009. The Red Sea, Coastal Landscapes and Hominin Dispersals. In M. Petraglia and J. Rose, eds. 2009. *The Evolution of Human Populations in Arabia,* 15-37. Dordrecht, Netherlands: Springer.

Barik, S.S., *et al.* 2008. Detailed mtDNA Genotypes Permit a Reassessment of the Settlement and Population Structure of the Andaman Islands. *AJPA* 136: 19-27.

Barton RNE, *et al.* 2009. OSL dating of the Aterian levels at Dar es-Soltan I (Rabat, Morocco) and implications for the dispersal of modern *Homo sapiens*. *Quarternary Science Reviews* 28: 1914-1931.

Bar-Yosef, O. 1992. Middle Paleolithic human adaptations in the Mediterranean Levant. In T. Akazawa, K. Aoki, and T. Kimura, eds., *The evolution and dispersal of modern humans in Asia,* 189-216. Tokyo: Hokusen Sha.

Behar, D., *et al.* 2008. The dawn of human matrilineal diversity. *J. Hum. Genet.* 82: 1130-1140.

Behar, D., *et al.* 2012. A Copernican reassessment of the human mitochondrial DNA tree from its roots. *AJHG* 90: 675-684.

Bengtson, J. D. 2006. A multilateral look at Greater Austric. *Mother Tongue* 11: 219.

Bengtson, J. D., & M. Ruhlen. 1994. Global Etymologies. In M. Ruhlen, *On the Origin of Languages: Studies in Linguistic Taxonomy*, 277-336. Stanford: Stanford University Press.

Birney, E., & J.K. Pritchard. 2013. Four makes a party. *Nature* 505: 32-34.

Blust, R. 1978. Review of Current Trends in Linguistics VIII: Linguistics in Oceania, ed. Thomas A. Sebeok. *Language* 54: 467.

Blust, R. 2009. *The Austronesian Languages*. Canberra: Pacific Linguistics, Research School of Pacific and Asian Studies, Australian National University.

Bomhard, A. 2006. *Reconstructing Proto-Nostratic: Comparative Phonology, Morphology, and Vocabulary*. Charleston, S.C.: Signum Desktop Publishing.

Bouckaert, *et al.* 2012. Mapping the Origins and Expansion of the Indo-European Language Family. *Science* 337: 957.

Bouzouggar, A., *et al.* 2007. 82,000 year-old shell beads from North Africa and implications for the origins of modern human behavior. *PNAS* 104, (4): 9964-9969.

Bouzouggar, A., & N. Barton. 2012. The identity and Timing of the Aterian in Morocco. Chapter 7, *Modern Origins: A North African Perspective and Vertebrate Paleobiology and Paleoanthropology* 2: 93-105.

Bowles, G.T. 1977. *The Peoples of Asia.* New York, Charles Scribner.

Brooks, A. 2006. Out of Africa by 77,000 BP. A brief synopsis of remarks made by Alison Brooks, preliminary to a larger paper later. *Mother Tongue* 11: 85-90.

Brown, K.S., *et al.* 2012. An early and enduring advanced technology originating 71,000 years ago in South Africa. *Nature* 491: 590-593. 22 November 2012.

Bruggemann, J.H., *et al.* 2004. Stratigraphy, palaeoenvironments and model for the deposition of the Abdur Reef Limestone: context for an important archaeological site from the last interglacial on the Red Sea coast of Eritrea. *Palaeogeography, Palaeoclimatology, Palaeoecology* 203, 3: 179-206.

Cann, R., M. Stoneking, & A.C.Wilson. 1987. Mitochondrial DNA and human evolution. *Nature* 329: 31.

Capell, A. 1956. *A New Approach in Australian Linguistics*. Sydney: Oceanic Linguistic Monographs.

Capell, A. 1975. The 'West Papuan Phylum': General, and Timor and Areas Further West. In S.A.Wurm (1975a), pp. 667-716.

Cavalli-Sforza, L.L., P. Menozzi, & A. Piazza. 1994. *The History and Geography of Human Genes.* Princeton: Princeton University Press.

Chu, J.Y., *et al.* 1998. Genetic relationship of populations in China. *PNAS* 95 (20), 11763.

Conti Rossini, C. 1927. Sui Linguaggi Parlati a nord de Laghi Rodolfo e Stefania. *Festschrift Meinhof*: 247-255.

Cowan, H.K. 1957a. A Large Papuan Language Phylum in West New Guinea. *Oceania* 28: 159.

Cowan, H.K.1957b. Prospect of a 'Papuan' Comparative Linguistics. *Bijdragen tot de Taal-, Land- en Volkenkunde* 113: 70-91.

Cremaschi, M., S. Di Lernia, & E.A.A Garcea. 1998. Some insights on the Aterian in the Libyan Sahara: chronology, environment, and archeology. *African Archeological Review* 15: 261-286.

Cruciani, F., *et al.* 2011. A revised root for the human Y chromosomal phylogenetic tree: the origin of patrilineal diversity in Africa. *AJHG* 88 (6): 814-818.

Day, M. 1986. *Guide to Fossil Man*. 4th edition. The University of Chicago Press. [p.108.]

Diebold, A. R. Jr. 1960. Determining the Centers of Dispersal of Language Groups. *International Journal of American Linguistics* 26.1: 1-10.

Dolgopolsky, A. 1998. *The Nostratic Macrofamily and Linguistic Palaeontology*. Cambridge: The McDonald Institute for Archaeological Research.

Donohue, M. 2002. *West Papuan Phylum. Revisiting the Genetic Status of the Yapen Island Languages*. Manuscript.

van Driem, G. 2008. The Shompen of Greater Nicobar Island: New linguistic and genetic data, and the Austroasiatic homeland revisited. *Mother Tongue* 13: 227-248.

Dyen, I. 1956. Language Distribution and Migration Theory. *Language* 32, 4: 611-626.

Dyen, I. 1965. A lexicostatistical classification of the Austronesian languages. *IJAL Memoir* 19. Bloomington, Indiana University.

Eddy, F.W., & Wendorf, F. 1999. *An Archaeological Investigation of the Central Sinai, Egypt*. Boulder: University Press of Colorado.

Endicott, P., *et al.* 2003. The genetic origins of the Andaman Islanders. *AJHG* 72: 178.

Endicott, P., *et al.* 2006. Multiplexed SNP typing of ancient DNA clarifies the origin of Andaman haplogroups among South Asian tribal populations. *PLoS One* 1(1): e81.

Erard, M. 2009. Linguistics: How many languages? Linguists discover new tongues in China. *Science* 324: 332-333.

Evans, N. & S.C. Levinson. 2009. The myths of language universals: Language diversity and its importance for cognitive science. *Behavioral and Brain Sciences* 32: 429.

Falk, D., *et al.* 2005. The Brain of LB1, Homo floresiensis. *Science* 308: 242.

Falk, D., *et al.* 2007. Brain shape in human microcephalics and Homo floresiensis. *PNAS USA* 104: 2513.

Fleming, H.C. 1991. A New Taxonomic Hypothesis: Borean or Boralean. *Mother Tongue: Newsletter of ASLIP* 14. (1 page)

Fleming, H.C. 1996. Looking to the West and North: Nihali and Kusunda Find Links. *Mother Tongue: The Journal*, Issue II, 67-74.

Fleming, H.C. 2002a. *Afrasian and its closest Relatives: The Borean Hypothesis*. Stanford University, at special conference to honor the memory of Joseph Greenberg. April 2002.

Fleming, H.C. 2002b. Poster on Borean shown at a week-long conference on "*Human Origins and Disease*" at Cold Spring Harbor Laboratories. Long Island, New York. Late October - early November, 2002.

Foley, W. 1986. *The Papuan Languages of New Guinea*. Cambridge: Cambridge University Press.

Forster, P. & C. Renfrew. 2012. Mother Tongue and the Y Chromosome. *Science* 333: 1390-1391.

Garrod, D.A.E., & D.M.A. Bate. 1937. *The Stone Age of Mount Carmel*. Oxford: The Clarendon Press.

Gatti, R. 1906-9. *Studi sul gruppo linguistico Andamanese-Papua-Australiano*. Bologna: Beltrami.

Gell-Mann, M., I. Peiros, & G. Starostin. 2009. Distant Language Relationship: The Current Perspective. *Journal of Language Relationship / Voprosy jazykovogo rodstva* 1: 13-30.

Gibbons, A. 2012. Turning Back the Clock: Slowing the Pace of Prehistory. *Science* 338: 189-191.

Gilead. I. 1991. The Upper Paleolithic Period in the Levant. *Journal of World Prehistory* 5: 105-154.

Gonder, M.K., *et al.* 2007. Whole mtDNA genome sequence analysis of ancient African lineages. *Molecular Biology and Evolution* 24: 757-768.

González, A. M., *et al.* 2006. The mitochondrial lineage U8a reveals a Paleolithic settlement in the Basque country. *BMC Genomics* 7: 124.

Grace, G. W. 1968. Linguistics. In Andrew P. Vayda, ed., *Peoples and Cultures of the Pacific. An Anthropological Reader*. Garden City (NY): The Natural History Press. [Pp.63-80. The Greenberg attribution is on page 79.]

Gray, *et al.* 2009. Phylogenies Reveal Expansion Pulses and Pauses in Pacific Settlement. *Science* 323: 479-483.

Green, R.E., *et al.* 2008. A Complete Neanderthal mitochondrial genome sequence determined by high-throughput sequencing. *Cell* 134: 416.

Green, R.E., *et al.* 2010. A draft sequence of the Neanderthal genome. *Science* 328: 710.

Greenberg, J.H. 1963. *The Languages of Africa.* (*International Journal of American Linguistics* 29.1, part 2.)

Greenberg, J.H. 1966. *The Languages of Africa.* 2nd ed. Bloomington: Indiana University Press.

Greenberg, J.H. 1971. The Indo-Pacific Hypothesis. *Current Trends in Linguistics* 8.

Greenberg, J.H. 1995. *Indo-European and Its Closest Relatives. The Eurasiatic Language Family*. Volume 1: *Grammar*. Stanford: Stanford University Press.

Greenberg, J.H. 2002. *Indo-European and Its Closest Relatives. The Eurasiatic Language Family.* Volume 2: *Lexicon*. Stanford: Stanford University Press.

Gregersen, E. 1972. Kongo-Saharan. *Journal of African Languages* 11: 69-89.

Hammer, M.F., *et al.* 1998. Out of Africa and Back Again: Nested Cladistic Analysis of Human Y Chromosome Variation. *Mol. Biol. Evol.* 14 (4): 427-441.

Hanihara, T. 1992. Dental and cranial affinities among populations of east Asia and the Pacific: the basic populations of east Asia. *AJPA* 88 (2): 163-182.

Harrod, J. 2011. Reflections on Archeology and Genetics: Selected Discoveries. *Mother Tongue* XVI: 1-10.

Haslam, M., *et al.* 2012. A southern Indian Middle Paleolithic occupation surface sealed by the 74 ka Toba eruption: Further evidence from Jwalapuram locality 22. *Quarternary International* 258: 148-164.

Hauck, *et al.* 2011. Mousterian technology and settlement dynamics in the site of Hummal (Syria). *Journal of Human Evolution* 61 (5): 519-537.

Henn, B.M., *et al.* 2011. Hunter-gatherer genomic diversity suggest a southern African origin for modern humans. *PNAS USA* 108: 5154-5162.

Hublin, J.-J., & R.G. Klein. 2012. Northern Africa could also have housed the source population for modern humans. *PNAS USA* 108: no.28, E277.

Hudjashov, G., *et al.* 2007. Revealing the prehistoric settlement of Australia by Y chromosome and mtDNA analysis. *PNAS USA* 104: 8726-8730.

Illič-Svityč, V.M. 1967. Materialy k sravnitel'nomu slovarju nostratičeskix jazykov. [Materials for a comparative dictionary of the Nostratic languages.] *Etimologija* 1965, 321-72. Moscow: Nauka.

Jacobs Z., *et al.* 2012. Single grain OSL dating at La Grotte des Contrebandiers ('Smugglers Cave'), Morocco: improved age constraints for the Middle Paleolithic levels. *Journal of Archaeological Science* 38: 3631-3643.

Jacobs, Z., *et al.* 2012. Single-grain OSL chronologies for Middle Palaeolithic deposit at El Mnasra and El Harhoura 2, Morocco. Implications for late Pleistocene human-environment interactions along the Atlantic coast of northwest Africa. *Journal of Human Evolution* 62 (3): 377-394.

James, H.V.A., & M.D. Petraglia. 2005. Modern human origins and the evolution of behavior in the Later Pleistocene of South Asia. *Current Anthropology* 56 (suppl.): S3-S27.

Jatsemirskij, S. 2011. Notes on Minoan Phonetics and Vocabulary. *Mother Tongue* 16: 35-62.

Karafet, T., *et al.* 2001. Paternal population history of East Asia: sources, patterns, and micro-evolutionary processes. *AJHG* 69 (3): 615-628.

Klein, R. G. 1999. *The Human career.* Chicago & London: University of Chicago Press. [p. 402, *et seq.*]

Klein, R.G. 2008. Out of Africa and the evolution of human behavior. *Evolutionary Anthropology* 17 (6): 267-281.

Kobusiewicz, M. 1999. Excavations at Sinai-20, The Split Rock Site, Zarnoq Locality. In: F. W. Eddy & F. Wendorf, eds., *An Archaeological Investigation of the Central Sinai, Egypt.* Boulder: University Press of Colorado, pp.193-207.

Kobusiewicz, M., *et al.* 2001. Reassessing Chronostratygraphic Position of the Split Rock Site, Sinai. In B. Gehlen, M. Heinen & A. Tilman, eds., *Archäologische Berichte* 14, Band I, *Zeit Räume,* Deutsche Gesellschaft für Ur- und Frühgeschichte: 227-236.

Kruskal, J.B., I. Dyen, & P. Black. 1973. Some Results from the Vocabulary Method of Reconstructing Language Trees. In I. Dyen, ed., *Lexicostatistics in Genetic Linguistics. Proceedings of the Yale Conference,* April 3-4, 1971. The Hague & Paris: Mouton. [Pp. 30-55.]

Lachance, J., *et al.* 2012. Evolutionary history and adaptation from high-coverage whole genome sequences of diverse African hunter-gatherers. ***Cell*** 150: 457-469.

Landau, M. 1984. Human Evolution as Narrative. *American Scientist* 72: 262-268.

Liu, W., *et al.* 2010. Human remains from Zhirendong, South China, and modern human emergence in East Asia. *PNAS USA* 107 (45): 19201-19206.

Marks, A.E. 1981. The middle Paleolithic of the Negev. In J. Cauvin & P.P. Sanlaville, eds., *Préhistoire du Levant*, pp.287-298. Paris: CNRS.

Matthey de l'Etang, A, P. Bancel, & M. Ruhlen. 2011. Back to Proto-Sapiens (Part One). The Inherited Kinship Terms *Papa, Mama,* and *Kaka*. In D. Jones and B. Milicic, eds. *Kinship, Language, and Prehistory: Per Hage and the Renaissance in Kinship Studies*. Salt Lake City: University of Utah Press, 29-45.

McBrearty, S. 2012. Sharpening the mind. Microlithic technology from 71,000 years ago. *Nature* 491: 531-532. 22 November 2012.

McBrearty, S. & A. Brooks. 2000. The revolution that wasn't: a new interpretation of the origin of modern human behavior. *Journal of Human Evolution* 39: 453-563.

McBrearty, S. & C. Stringer. 2007. The coast in colour. *Nature* 449: 793-794.

McCall, Daniel. 2007. Review of Anthony (2007). *Mother Tongue* 12: 215-222.

McDougall, I., F.H. Brown, & J.G. Fleagle. 2005. Stratigraphic placement and age of modern humans from Kibish, Ethiopia. *Nature* 433: 733-736.

McElhanon, K.A. & C.L. Voorhoeve. 1970. The Trans New Guinea Phylum: Explorations in Deep Level Genetic Relationships. *Pacific Linguistics* B16.

Mercier N., *et al.* 1999. Thermoluminescence dating of a Middle Paleolithic occupation at Sodmein Cave, Red Sea Mountains (Egypt). *Journal of Archaeological Science* 26 (11): 1339-1345.

Meyer, M., *et al.* 2012. A High-Coverage Genome Sequence from an Archaic Denisovan Individual. *Science Express* 10.1126 / *Science.* 1224344 (2012).

Mijares, A.S., *et al.* 2010. New evidence for a 67,000 year old human presence at Callao Cave, Luzon, Philippines. *Journal of Human Evolution* 59 (1): 123-132.

Mishmar, D., *et al.* 2003. Natural selection shaped regional mtDNA variation in humans.

PNAS USA 2003, 100: 171-176.

Moyer, C. 2003. *The Organisation of Lithic technology in the Middle and Early Upper Palaeolithic Industries of Haua Fteah, Libya*. Doctoral Dissertation, Corpus Christi College, Cambridge.

Murdoch, G.P. 1959. *Africa: Its Peoples and Their Culture History*. New York: McGraw-Hill. [p. 259.]

Northrop, F.S.C. 1947. *The Logic of the Sciences and Humanities*. New York: Macmillan & Co.

Olivieri, A., *et al.* 2006. The mtDNA Legacy of the Levantine Early Upper Palaeolithic in Africa. *Science* 314: 1767-1770.

Oppenheimer, S. 2009. The great arc of dispersal of modern humans: Africa to Australia. *Quarternary International* 202: 2 (2009).

Oppenheimer, S. 2012. Out-of-Africa, the peopling of continents and islands: tracing uniparental gene trees across the map. *Phil. Trans. R. Soc.* B2012 367, 770-784.

Orel, V. 1995. Semitoxamitskij, sinokavkazskij, nostraticheskij [Semito-Hamitic, Sino-Caucasic, Nostratic]. *Moskovskij lingvisticheskij zhurnal* 1: 99-116.

Pagel, M. 2000. Maximum Likelihood Models for Glottochronology and for Reconstructing Linguistic Phylogenies. In C. Renfrew, A. McMahon, & R.L. Trask, eds. *Time Depth in Historical Linguistics.* Cambridge: McDonald Institute for Archaeological Research, vol. 1, 189-207.

Pawley. A., *et al.* 2005. *Papuan Pasts. Cultural, Linguistic and Biological Histories of Papuan-Speaking Peoples*. Canberra: Australian National University.

Pedersen, H. 1931. *Linguistic Science in the Nineteenth Century*. Cambridge, Massachusetts.

Pereira, *et al.* 2010. Linking the sub-Saharan and West Eurasian gene pools: maternal and paternal heritage of the Tuareg nomads from the African Sahel. *European Journal of Human Genetics* 18: 915-923.

Perreault, C., & S. Mathew. 2012. Dating the Origin of Language Using Phonemic Diversity. *PLoS One* 7: 1-8.

Petraglia, M.D., *et al.* 2007. Middle Paleolithic assemblages from the Indian subcontinent before and after the Toba super eruptions. *Science* 317: 114-116.

Petraglia, M.D., *et al.* 2010. Out of Africa: new hypotheses and evidence for the dispersal of Homo sapiens along the Indian Ocean Rim. *Annals of Human Biology* 37: 288.

Petraglia, M.D., *et al.* 2011. Middle Palaeolithic occupation on a Marine Isotope Stage 5 lakeshore in the Nefud Desert, Saudi Arabia. *Quarternary Science Reviews 30* (13-14): 1555-1559.

Petraglia, M.D., *et al.* 2012. Hominin Dispersal into the Nefud Desert and Middle Palaeolithic Settlement along the Jubbah Palaeolake, Northern Arabia. *PLoS ONE* 7 (11): e49840.

Petraglia, M.D. & J. Rose, eds. 2009. *The Evolution of Human Populations in Arabia.* Dordrecht, Netherlands: Springer. [pp. 15-37.]

ten Raa, E. 1962. Personal letter, concerning East African ethnography.

Rasmussen, M., *et al.* 2011. An aboriginal Australian genome reveals separate human dispersals into Asia. *Science* 334: 94-98 / *Science Express* 10, 1126.

Reich, D., *et al.* 2010. Genetic history of an archaic hominin group from Denisova Cave in Siberia. *Nature* 468: 1053-1060. [especially p.1057.]

Reich, D., *et al.* 2011. Denisovan admixture and the first human dispersals into Southeast Asia and Oceania. *AJHG* 89: 1-13.

Reich, D., *et al.* 2012. Reconstructing Native American Population History. *Nature* 488: 370-375.

Richter, D., *et al.* 2010. New chronometric data from Ifri n'Ammar (Morocco) and the chronostratigraphy of the Middle Palaeolithic in the Western Maghreb. *Journal of Human Evolution* 59: 672-679.

Rose, J. 2004a. The question of Upper Pleistocene Connections between East Africa and South Arabia. *Current Anthropology* 45: 551.

Rose, J. 2004b. New evidence for the expansion of an Upper Pleistocene population out of East Africa, from the site of Station One, Northern Sudan. *Cambridge Archaeological Journal* 14: 205-216.

Rose, J. 2007. The Arabian Corridor Migration Model: archaeological evidence for homonin dispersals into Oman during the Middle and Upper Pleistocene. *Proceedings of the Seminar for Arabian Studies* 37, 1.

Rose, J. 2010. New light on Human Prehistory in the Arabo-Persian Gulf Oasis (with Comments). *Current Anthropology* 51, 6: 849-883.

Rose, J., *et al.* 2011. The Nubian Complex of Dhofar, Oman: an African middle stone age industry in southern Arabia. *PLOs ONE* 6, e28239.

Ross. M. 2005. Pronouns as a preliminary diagnostic for grouping Papuan languages. In Andrew Pawley, *et al.*, eds. *Papuan Pasts: cultural, linguistic and biological histories of Papuan-speaking peoples*. Canberra: Pacific Linguistics. Pp. 15-66.

Ruhlen, M. 1991. *A Guide to the World's Languages*. Postscript. Stanford: Stanford University Press.

Scally, A., & R. Durbin. 2012. Revising the human mutation rate: implications for understanding human evolution. *Nature Reviews* 13: 745-750.

Scerri, E. M. L. 2012. The Aterian and its place in the North African Middle Stone Age. *Quarternary International* (2012), http://dx.doi.org/10.1016/j.quaint.2012.09.008.

Schlebusch, C.M., *et al.* 2012. Genomic Variation in Seven Khoe-San Groups Reveals Adaptation and Complex African History. *Science* 338: 374.

Schmidt, W. 1926. *Die Sprachfamilien and Sprachenkreise der Erde.* Heidelberg.

Shea, J. J. 2008. The Middle Stone Age archeology of the Lower Omo Valley Kibish formation: excavations, lithic assemblages, and inferred patterns of early *Homo sapiens* behavior. *Journal of Human Evolution* 55 (3): 448-485.

Shea, J. J. 2009. The Impact of Projectile Weaponry on Late Pleistocene Hominin Evolution. In J-J. Hublin & M.P. Richards, eds. *The Evolution of Hominin Diets: Integrating Approaches to the Study of Palaeolithic Subsistence*. New York: Springer Science, pp. 823-46.

Shea, J. J. 2011a. Homo sapiens is as Homo sapiens was? *Current Anthropology* 52: 1-35.

Shea, J. J. 2011b. Refuting a Myth about Human Origins. *Scientific American* 99.2: 128-135.

Shea, J. J., 2012. *Science News*, August, 2012. p.26. [No title, as such.]

Shea, J. J., J.G.Fleagle, & Z. Assefa. 2007. Context and Chronology of Early *Homo sapiens* Fossils from the Omo Kibish Formation, Ethiopia. In P. Mellars, *et al.*, eds. *Rethinking the Human Revolution*. Oxford: Oxbow Books, pp. 153-164.

Smith, J.R., *et al.* 2007. New age constraints on the Middle Stone Age occupations of Kharga Oasis, Western Desert, Egypt. *Journal of Human Evolution* 52: 690-701.

Smith, T.M., *et al.* 2007. Earliest evidence of modern human life history in North African early *Homo sapiens*. *PNAS USA* 104 (15): 6128-6133.

Soares, P., L. Ermini, *et al.* 2009. Correcting for purifying selection: an improved human mitochondrial molecular clock. *AJHG* 84, 6: 740-59.

Soares, P., A. Achilli, *et al.* 2010. The archaeogenetics of Europe. *Current Biology* 20.4: R174-83.

Starostin, S.A. 1988. Nostratic and Sino-Caucasian. In V. Shevoroshkin, ed., *Explorations in*

Language Macrofamilies. Bochum: Brockmeyer. 42-66.

Steinberg, A., & C.E. Cook, 1981. *The Distribution of the Human Immunoglobulin Allotypes*. Oxford University Press.

Sugden, A. M. 2011. Diversity Takes Time. *Science*, 332 (6033): 1011.

Swadesh, M. 1960. *Tras las huella lingüística de la prehistorica*. (Suplementos del Seminario de Problemas Científicos y Filósoficos, 2a Serie, 26: 97-145.) Mexico City: Universidad Nacional Autónoma de México.

Szabo, B.J., *et al*. 1995. Ages of Quarternary pluvial episodes determined by uranium-series and radiocarbon dating of lacustrine deposits of Eastern Sahara. *Palaeogeography, Palaeoclimatology, Palaeoecology* 113: 227-244.

Thangaraj, K., *et al*. 2003. Genetic affinities of the Andaman islanders, a vanishing human population. *Current Biology* 13: 86-93.

Thangaraj, K., *et al*. 2009. Reconstructing the origin of Andaman islanders. *Science* 308: 996.

Tishkoff, S., *et al*. 2009. The genetic structure and history of Africans and African Americans. *Science* 324: 1035-1044.

Tishkoff, S., & S.M. Williams. 2002. Genetic Analysis of African Populations: Human Evolution and Complex Disease. *Nature Reviews: Genetics* 3: 611-621.

Trombetti, A. 1905. *Unita d'Origini dei Linguaggio*. Bologna: Beltrami.

Usher, T. 2006. Paper given at a conference on the "Australoid Realm." Harvard University, October, 2006.

Valladas, H., *et al*. 1998. GIF laboratory dates for Middle Paleolithic Levant. In T. Akazawa, K. Aoki, & O. Bar-Yosef, eds. *Neandertals and modern humans in western Asia*. New York: Plenum Press, pp. 69-76.

Van Beek, G.W., G.H. Cole, & A. Jamme. 1964. *An Archeological Reconnaissance in Hadhramaut, South Arabia – A Preliminary Report*. Washington: Smithsonian Institution.

Van Peer, P. 1998. The Nile Corridor and the Out of Africa model: an examination of the archaeological record. *Current Anthropology* 39 (suppl.): S115-S140.

Van Peer, P., *et al*. The Early to Middle Stone Age transition and the emergence of modern human behavior at site 8-B-11, Sai Island, Sudan. *Journal of Human Evolution* 45: 187-193

Verdu, P., *et al*. 2009. Origins and genetic diversity of pygmy hunter-gatherers from Western Central Africa. *Current Biology* 19 (4): 312-318.

Vermeersch, P.M., P. van Peer, & E. Paulissen. 2010. Chert Quarrying, Lithic Technology, and a Modern Human Burial at the Palaeolithic Site of Taramsa Upper Egypt. *Egyptian Prehistory Monographs*. Leuven: Leuven University Press.

Voorhoeve, C.L., & L. Clemens. 1975. Central and Western Trans-New Guinea Phylum Languages. In S.A. Wurm (1975a), pp. 345-460.

Walter, R.C., *et al*. 2000. Early human occupation of the Red Sea coast of Eritrea during the last interglacial. *Nature* 405: 65-69.

Whitehouse, P. 1997. External Relations of Nihali and Kusunda. *Mother Tongue* 3: 4-44.

Whitehouse, P., *et al*. 2004. Kusunda: An Indo-Pacific language in Nepal. *PNAS* 101: 5692.

Wiens, J. J., Pyron, R. A., & Moen, D. S. 2011. Phylogenetic origins of local-scale diversity patterns and the causes of Amazonian megadiversity. *Ecology Letters* 14: 643-652.

Wurm, S.A., ed., 1975a. *New Guinea Area Languages and Language Study*, Vol.1: *Papuan languages and the New Guinea linguistic scene*. (*Pacific Linguistics* C38.) Canberra: Research School of Pacific and Asian Studies, Australian National University.

Wurm, S.A. 1975b. Possible wider connections of Papuan languages: Torres Straits and north

Australia. In S.A. Wurm (1975a), pp. 915-24.

Wurm, S.A. 1982. *Papuan Languages of Oceania*. Tübingen.

Yellen, J., *et al.* 2005. The archeology of Aduma Middle Stone Age sites in the Awash Valley, Ethiopia. *PaleoAnthropology* 10: 25-100.

Zhong, H., *et al.* 2011. Extended Y Chromosome Investigation Suggests Postglacial Migrations of Modern Humans into East Asia via the Northern Route. *Molecular Biology and Evolution* 28 (1): 717-727.

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

On the Origin of Milyan Verbs

Vitaly Shevoroshkin
University of Michigan

Introduction

This paper is a follow-up to my publication *On the Origin of Milyan Nouns* (MT XVI, 2011). Since I intend to finish writing at some point (hopefully, soon enough) a book on Milyan (primarily, a dictionary with elaborate entries) I try to pay special attention to numerous ties which exist between a given Mil. verb and other words in Mil. passages; as a result, a coherent multi-level structure slowly continues to emerge.

The main part of this paper is *An Annotated List of Milyan Verbs*, referred to as (the) **List**.

About a half-century ago J. Rasmussen concluded that the Lyc.-Mil. letter *q* reflects a labiovelar because it never appears before labial sounds (no **qu*). As it became clear later, mostly thanks to D. Schürr, the Lyc.-Mil. obstruent *q* [x^w] had originated both from the IE. labial laryngeals (**h^w*, **hw*) and from a labial stop **g^wh*. Today we may add a voiced labial fricative *γ* [$γ^w$] (which seems to be absent in Lycian proper) to the Mil. phonetic inventory as an archaic voiced counterpart to *q*, cf. *mrγγa-* 'dark [divinity]' : Ht. *margwa-* id. vs. Lyc. **mrbba-* : CLuv. *marwa-* (all to IE. **$merg^w$-* 'dark'), amd *layre/i-* to Ht. *lah(h)ura-* (a stand / container used in offerings; cf. DLL: 119). - This situation refers to the state of the Mil. phonetics as it is reflected in both known inscriptions.

Both Mil. texts are poetic, providing several exx. of a phonetic variation *-t/d-* in the vb. endings (3-sg.), cf. *sla-ti* & *sla-di*; *χba-ti* & *χba-de* [vb. form, not a noun]; the numerous Lyc. inscriptions contain only very few such variations. - Both Piχre (author of a relatively short text TL 55) and Xerẽi (author of TL 44 where the Mil. part is represented by 37 strophes, in 44c-d) have used the above variety of verbal endings for occasional ornamentation of their passages. For instance, each of two opening strophes of 44c (Mil. part) shows four precisely rhyming verbs (2 in each strophe): *χb-a-ti* 'assigns' [*χb-a-ti*, not *χbadi*, because the subsequent verb ends in *-a-ti*] : *pin-a-ti* 'presents' in 44c34-36 (subj.: [god] Trqqiz); note also a structurally similar pair of verbs *sl-a-ti* 'awards' : *zaz-a-ti* 'arranges' in the preceding strophe (subj.: [god] Natri). - Actually, Xerẽi seems to follow here Piχre's pattern, - this latter has authored two strophes with rhyming verbs.

In addition to the above strophes 44c.I-II (which open the Mil. text of the Xanthos stele), five more of Xerẽi's strophes contain the same sentence type: two neighboring sentences, each with a 3-sg. verb, both forms rhyming. The following endings have been used by Xerẽi in the rhyming verbal forms in these five strophes: *-(a)ti* (3-sg. pres.); *-te* (3-sg. pret.); *-tu* (3-sg. imp.); the rhyming verb pairs from the five strophes in question are as follows:

1) *pss-a-t[i]* … *(ne)* *mrsχχ-a-ti* 'delivers … (not) cheats' (subj.: *zajala* 'tax-payer')

2) *puke-ti* ... *kupri-ti* 'rescues' (subj.: [god] Natri of Kaunos) … 'favors' (subj.: [god] Natri of Turaχssa)
3) *χusti-te* 'rushed (the assets to …') (subj.: 'he [= Xerẽi]') ... *padre-te* 'presented' (subj.: the Lyc. ruler Xeriga)
4) *zaza-ti* 'arranges' (subj.: *ñtuwitẽni*-Xerẽi, the Lyc. commander-in-chief) … *tetbe-ti* 'damages, may damage' (subj.: *mutala* 'strong man')
5) *qre-tu* ... *pu-tu* 'let him fill (Trqqiz's vessel)' … 'let (him) adjoin [the statue of the god] Zrppedu / Sarpedon to [those of] the assembly-gods)'; subj.: [god$^{?}$] Trlluba.

There exist certain phonetic features, both in Lyc. and Mil., which are present in Ht. but not in Luv. (as shown by the CLuv. data), one of them is an immunity to Čop's Law, cf. Lyc. **tabaha-* 'sky' : CLuv. *tappassa-*; Mil. *medu* (acc. sg.) : CLuv. *maddu-*; Mil. *edul-i* (d. sg.) 'for harm' : CLuv. *adduwal-*; Mil. vb. *pdura- / padre-* 'bring, present', based on the from **padur* : CLuv. **paddur-* 'tray' (see List).

Note a pattern *-Cu* : *-CbV* in closely related Lyc. and / or Mil. words which can be projected on the Anat. level: *-Cu* : *-CbV* < **-Cu-* : **-CwV*, cf. five exx.:

(a) Mil. *medu* (a treat) : Lyc. adj. *medbije-* : CLuv *maddu-* 'wine', adj. *madduwiya-*
(b) Lyc.(-Mil.) *tu-* ('2', as in Lyc. *tu-pm̃me*) : Mil. *tbi-* '2'
(c) Mil. *elu-* : *alba-* 'libate' : Ht. nominal stem *alwa-* 'sorcery' (or sim.); see List
(d) Ht. *šāru-* 'booty' (:IE. **soru-* in MIr. *serb* 'robbery') : Mil. **zrbba-* in *zrbbla-* id.
(e) Kappad. *-hsu* (in PNN; cf. Ht. *hassu-*) : Lyc. *χahba-* 'grandchild' < **hass(u)wa-*.

Both recent dictionaries of Lyc.-Mil. (DLL [2004] and GL [2007) still show, on many occasions, an approach to the above *q* as a sound, very similar to *χ*, the regular Lyc.-Mil. 'laryngeal' which matches genetically CLuv. / Ht. *h(h)*. This situation seriously hampers our understanding of the origin of Lyc. and Mil. words, containing *q*, which are frequently tied to words wih *χ* genetically, though (as we now understand) they are not mutually related (by the way, a percentage of words with the initial *q-* in two Milyan inscriptions [29 words in DLL] is, for some reason, much higher than that in the numerous Lyc. inscriptions, some being relatively long [11 words in DLL]).

Another problem we have to deal with is caused, first of all, by an occasional lack of word-division marks in Mil. text; as a result, we may find in dictionaries a 'noun' which is, actually, a sequence of two or more neighboring words; an attentive text analysis may provide a correction:

f) *ssepsse-*, allegedly represented by acc. sg. *ssepssẽ*; in reality, we have two neighboring words: d. pl. *sse* < *sese** (cf. d. sg. *sesi*; see **sese-* 'distribute' in the List) and g. pl. *pssẽ* (cf. *pssa-* 'deliver' in the List); note a similar case: *lle* <*lele** to *leli* (d. sg.; acc. sg.); related to the vb. *leli-* 'tell, narrate'.
g) *muwilada-* as in d. pl. *muwilade*; in reality, we deal here with d. sg. *muwi* ('for invigoration', to *muwa-* : CLuv. *mu-muwa-* 'invigorate') + *la-de* '(he)

took' (+ acc.-sg. phrase *ali ... sebe pasbã* 'command and troops / detachment'; cf. *wijedri ... sebe pasbã* id.; in a reversed order: *sebe pasbã ... sebe χñtabu*); cf. sub *la-* 'take'.

A serious obstacle to a proper interpretation of a number of Mil. passages can also be caused by our negligence toward the original spelling of Mil. words, - and this may include phrases in which word-division marks are being persistently ignored:

h) a form *ãzisse* is used in dictionaries for a clearly spelled word-pair *ãzi: sse*, - just because a form *ãzisẹ* (whose meaning is still unknown) exists in Lycian; in reality, we deal with two words (as shown by the word-division mark), n. sg. *ãzi* and d. pl. *sse*; cf. *ãzi-* 'supply'? sub **ai-* 'take' in the List; for *sse* see ex. f.

i) *uwe memleje* is a traditional spelling instead of the correct *uweme:* (noun in d. pl. + adj. in d. pl.) with a very clear word-division mark after *uweme*. The correct spelling leads to an important identification of the structure of the phrase *uweme: mleje* as 'noun in d. pl. + adj. in d. pl.', - and of the meaning of the noun *uwemi-* as 'drink, libation' (:HLuv. participle *uwami-* to **uwa-* 'drink' : Mil. *uwa/e-* 'libate'); the attr. adj *mleje* is a form of the adj. *mlei(je)-* 'pertaining-to-meal(s) / offering(s)'; cf. similarly structured adjectives *murei(je)-*, *nei(je)-*, *pssei(je)-*, etc.; this type of Lyc.-Mil. adjectives has been identified by Melchert.

I would like to mention here a problem, connected with the citation in the dictionaries of inanimate nouns in *-m̃* <**-men*, namely, *alb-m/m̃* 'beverage' (*-m* from *-m̃* before vowels), *masχχ-m̃* 'grant', *χez-m̃* 'allotment, share'. There seems to be no ground to assert that the form *χez-m̃* (44d.36) may originate from **χezma*, and there is no participle **χezme/i-* [by the way, we would rather expect **χezm̃ma* and **χezmm̃e/i-*, cf. an unrelated verbal noun *qezm̃mi-*]. We rather have an opposition *χez-m̃* 'allotment, share' (inanim.; introduced by inanim. pron. *-de*) : *χzzãta-* id. (anim.) [not 'Xanthos'!]; see sub **χzza-* 'allot' in the List. - As for the participles with the suff. *-m̃* < **-mi-*, cf. *eim̃* sub **ai-* 'make' (see the List).

In the exx. j-w, verbal forms are listed which, unfortunately, are identified in the dictionaries either as belonging to a non-related verbal base, or (very frequently) as nominal forms, or (very seldom) as adverbs or pronouns. - Since the verbs in question are present in the List I'm providing here a very laconic enumeration:

j) *asχχa* is not a 1-sg. pret. of *as-* but a 2-sg. imp. of *asχχa-* (cf. 3-sg. *as[χχa]-ti*)
k) *elu-wi* is a 1-sg. pres. (for *elu-* cf. *welpu-*), not a d.-l. sg. of a noun; cf. *alba-*
l) *epe* in 55.5 is a 2-sg. imp. (cf. *χi*, ex. h) [not an adv.], cf vb. form *ep-di* 'takes'
m) *etrqqi* is a 2-sg. imp. of a vb. *etrqqi-* (type: *zr-qqi-*), not a noun in d.-l. sg.
n) *tetbe-ti* is a 3-sg. pres. vb. [not a noun in n. sg.]; the preceding *apñtadi* is ins. in *-di* (to *a/ep-ñt-a-* 'taking, booty'; cf. vb. *ep-di*); synon.: *lelebe-di*, *q/χidrasa-di*, *sukre-di*.
o) *(t-)m̃qri-s-ñte* is a 3-pl. pret. form (iter. *m̃qri-s-* 'ration'), not a noun in d.-l. pl.
p) *tu* (5x) is a 2-sg. imp. of *tu-* 'place' [cf. Lyc.-Mil. iter. *tus-*], not a pron. *-tu*.

q) *χi* in 55.5 is a 2-sg. imp. 'sacrifice'; noun *χi* (d.-l.sg.) appears in 44c.35
r) *χba-de* is a vb. form (3-sg. pret.), not a noun in d. pl.; cf. *χba-ti* 'assigns' (*-d* /*t-*)
s) *zi-u* is a 1-sg. pres. form of *zi(je)-* 'provide, award', not an acc.-sg. of *zia-*
t) *slama* / *slãma* is a 2-sg. imp. as required by the text, nowhere a noun.
u) *murẽne-di* is a 3-sg.-pres. vb. (suff. *-ẽn-e-*; cf. v), not a noun *murẽn-* in abl.-ins.
v) *qelẽnẽti* is either a 3-pl. vb. form or a gerund-like form ('when …-ing'), not an adj.
w) *(a)stte* is a vb. form (3-sg. pret. of *as-* 'make'); there is no noun '*nestte*'.

A number of nominal forms are interpreted in dictionaries as verbs; occasionally, an incorrect word demarcation is proposed. For instance, the form *mryydi* (3x) is considered as a vb. form (3-sg. pres.) though the text analyzis clearly shows that we deal here with a noun in l. sg., possibly 'above the dwelling of the *mrγya*-divinities of the Netherworld', or sim. (*mrγya-* to Ht. Marg(u)wa-, above); the sepulchers of Xerẽi (44c.32), Xeriga (44c.37), Piχre (44d.48-49: Xerẽi's visit to Piχre's tomb 'in the precinct of Wzzaije/Antiphellos') were situated in such locations; cf. MT XVI: 81).

Seven more exx. of an incorrect identification of nominal forms as verbal (occasionally along with a false demarcation of words, as in [*trqqñtasa-ti* ... for *trqqñtasa* + *ti:k(a)* + *di<j>a*]):

x) *a/epñta-di* is abl.-ins. (to *a/ep-ñt-a* 'taking, booty'), not a vb. form in 3-sg. pres.
y) *sukre-di* is not a vb. but an ins. in *-di.* of *sukre/i-* (a beverage) [noun, not adj.]
z) *ermede* is a noun (in dat.), in a rite enumeration: 44c.62 *tali: ermede leli: tulijeli*
aa) there is no 3-sg. verb '*ije-ti*' in 44c.61; we have d. pl. phrase *edije tike*
ab) *trqqñtasa* is an adj. in 55.8 *trqqñtasa ti:kdi<j>a* ... *χrbbla[ta]*; no vb. form '*trqqñtasa-ti* '; similar in 44c.61-62: *edije tike* (d. pl.) ... *χrbblatã: trqqñtasi* (acc. sg.)
ac) no vb. '*χñtaba-tu*' in 44d.71; DS reads: *χñtaba tutl-tu* (n. sg. + 3-sg. imp.).
ad) there is no vb. base *ta-* in *a/epñta-di* (= abl.-ins., ex. x); Lyc. *ta-* 'put, place' always matches Mil. *da-*, cf. related nouns: Lyc. *ñta-ta-* & Mil. *ñta-da-* 'sepulcher'.

- I am deliberately not paying attention to many improper identifications of case endings: for instance, all. or dat. in *-a* is frequently explained as acc. coll.; in such situations, an attentive analysis of broader context may be of help.

The following comparison shows that synonymous verbs *(ẽnẽ) sla-* and *zi(je)-* 'provide, award' are used in sentences with an acc.-obj. phrase (which, as a rule, denotes a military unit), and an ins. case (which probably refers to drinks, used as an award for warriors, or a treat for celebrants): *qrbble-di* to *qrbble/i-* 'drinking vessel, goblet', *sukre-di* to *sukre/i-* (some beverage), etc; for nouns see my *Annotated List of Milyan Nouns* in TM 2011. - For *qrbble/i-* cf. d.-sg. *prijãm-i* ...*qrbbl-al-i* '(take the [statue of the god] Qaja of Phellos) for a cherished libation' (or sim.) in an instruction, uttered by Trqqiz (cf. iter. *kiki-ti* 'recite' about four Trqqiz;s instructions).

subj.	verb	acc. obj.	instr.	d. obj. (award grounds)
(1) [Trqqiz]	*ẽnẽ slatu*	*atrala*	*wesedi*	*pasñte...erbbi km̃qi... lbbewel-i*
	let him award	detachment	w. good ones$^?$	for [P.'s] protection, fight, spoils
(2) *natri*	*sla-ti*	*pasb-ã... ñtab-u*	*χustte-di*	*sbirt-e ... zireim-a*
Natri	awards	troops ... leader	w. quick ones$^?$	for shares for storage(s)
(3) [Xerẽi]	*zi-u*	*pasb-u*	*sukre-di*	*pttil-i ... χustt-i ... qidra-la*
	I'll award	troops	w. libations	for swiftness, rushing, raiding
(4) [men]	*ẽnẽ slãtu*	*χbadiz*	*qrbbledi*	
	let them provide	Xanthians	w. goblets	

The grounds for which a protective unit is to be awarded in the text TL 55.6 (ex. 1) match, in principle, the appropriate notions in Xerẽi's text TL 44d.49 (ex. 3); this shows that Xerẽi has based his strophe on that of Piχre. On the other hand, Xerẽi clearly stresses martial qualities which contain a semantic component 'swiftness' whereas Piχre uses only one word of this class, namely, *erbbi* 'for fight(s)'. There are no other words of this type in the whole text 55, whereas Xerẽi uses many nouns, adjectives, and verbs which refer to swift actions.

An Annotated List of Milyan Verbs

a- 'make' (: Lyc. *a-* id.) as in 44d.36 *-d(e) a-du* 'let one / him make this [= a feast / offering$^?$] ...'; a form *da-du* 'let one/him put/place' is less likely since the verb *da-* requires a dir. obj. (see sub *da-*). - Cf. related verbs _*ai-_, *as-* below.

_*ai-_ (or _*ei-_ ?) 'make' in a participle *eim̃* < _ai-mi-*_ used as a predicate 'is made / has been made'$^?$ in 44d.55-56 (Xerẽi stresses his affection for the military): ... *ẽmu: me-uwe: ãzi: sse[:] χuzruwãta: eim̃: waχssa* (all. or d. pl. vs. d. sg. *waχsi*) 'Now, for me, a supply$^?$ (*ãz-i-*) is / has been made for the distributions (d. pl. *sse* < _ses-e*_ : d. sg. *ses-i* in 44d.66 [not an adj.]; see _*sese-_) to protecting / protective guards'; for *ãzi* (n. sg.) 'supply' cf. IE. _*h_1n(e)k̂-_ 'obtain, take' (cf. LIV^2: 250) > Slav. _*nes-_ 'carry'? [emendation *ãzi{:}sse* = '*ãzisse*' is unnecessary: we have two words, n. sg. *ãzi* and d. pl. *sse*, not one]. - The auslaut *-m̃* originates from _*-mi-_ (suff. of a participle) in the above form *eim̃*; different: inanim. nouns in *-m̃* _*-men_ (*masχχm̃* 'grant', *albm̃* 'libation', *χezm̃* 'allotment'); see *nẽnije-* 'drive, send', *alba-* libate, _*zza-_ 'allot'.

alba- 'libate, intoxicate'$^?$ (to *elu-*, below) : Ht. *alwa-* 'sorcery'$^?$ in *alwa-nza-* 'affected by sorcery', *alwa-nz-ahh-ant-* 'bewitched', etc. (from IE., cf. HED 1/2: 46f.); note other IE. languages: Latv. *aluôt* 'be distraught', etc. (HED 1/2: 43ff.) - Related nouns seem to denote beverage(s) / vessel(s): direct objects *albm* (*-m* for *-m̃* before a vowel) / *(a)lb[m̃]* (*-m̃* < _*-men_, suff. of inanim. nouns); *albã*:; *albãma* (acc. coll.); *albrãna* (acc. coll.; Trqqiz's vessels?); n. sg. *(a)lbijẽi* 'supervisor of drinks'. - 44d.42-43 (Xerẽi awards officials in Tralles): *tralije: wijedri-be: albaχã : m̃qr[i/e er]eime* (d. pl.) 'I libated the authority / command for rations / shares (d. sg. *m̃qr[i]* or d. pl. *m̃qr[e]*) for Trallian storages' (nasalization in the verbal ending *-χã* indicates acc. sg. anim. *wijedri* [wijedrĩ]). [There is no *m̃qreime*; three signs are lacking between the letter sequences *m̃qr-* and *-eime* in this damaged line]. - Cf. *m̃qri(s)-*.

as- 'make, do' (iter. to *a-*, above) : Lyc. *as-* id. (DLL: 113 [there is no 1-sg. pret. *as-χχa*; cf. vb. *asχχa-* 'secure, provide', below]). - 44d.50-52 (Xerẽi seems to speak about collecting harvest taxes) *urttu: qelid[e]li: kibe-i me-i: per(e-)epñ ... ne (a)stte mlati* 'A harvest$^{?}$ (adj. *q.*) tax (acc. sg. *urttu*), now here now there, now soon(er) now late(r), one made / delivered it at / to the precinct (d.-l. sg. *mlati*)'. [The form *(a)stte* is the only verb in this strophe; accordingly, there is no noun *nestte*].

****as-*** (or **es-* ?) 'sit' : Lyc. **ah-* (or **eh-* ?) id. : Ht. *eš- / aš-* 'sit (down), reside, (trans.) settle' (EDH: 252f.); from IE.; see **asa-*, next.

****asa-*** (or **ese-* ?) 'remain, rest; settle', ultimately to **as-* (or **es-*) 'sit', above (cf. DLL: 2 and 115; EDH: 252f); **asa-* is probably reflected in *asa* (all. or d. sg.) 'staunchness', *asata-* (or *esete-* ?) 'peace, rest, well-being'$^{?}$ (lit. 'stability') in d. sg. *eseti* : Lyc. d. pl. *ahata*, to CLuv. **aššatt(i)-* 'peace'$^{?}$ to Anat. **āss-* 'remain, stay' (cf. CLL: 35). - An opposition *asata-* (Lyc. *ahata-*) 'peace' vs. Lyc.-Mil. *erb(b)e/i-* 'war, battle' is seen in Mil. derivates *esete-si-* 'peaceful' vs. *erbbe-si-* 'war-like' = divine epithets, cf. 44d.12-13 *trqqiz: esetesi-[k]e er[b]besi-ke* 'Trqqiz [who is] both peaceful and war-like' [the subsequent adj. *lusasi* 'fiery' refers to d. sg. *esẽnẽ-mla* 'blood-sacrifice']. - In the Lyc. passage 29.3-4, the noun *erbbe-* 'war, battle' is clearly opposed to *ahata-* 'peace, well-being' [*erbbe* is here n. sg., not d.-l. sg. or d.-l. pl.] : *se ñtemlẽ: qastte teli: erbbe: me ti ñtẽmlẽ: przze: astte [....] se-j-ahata: astte: se tijala* (+ subj. phrase) 'And when (*teli*) a war / battle destroyed the altar$^{?}$ ('sacrificial installation'$^{?}$, DLL: 46), - now, who (*ti*) [later$^{?}$] made / rebuilt the altar$^{?}$ for the forefathers (d. pl. *przze*, recepients of offerings, DLL: 52) and made (it) both for peace / well-being (d. sg. or coll. *ahata*) and for contribution(s) / tribute-delivery$^{?}$ (d. sg. or coll. *tijala*)', [is/was] (+ subj. phrase)'; for *tijala* 'payments' cf. DLL 67 (to **ti- / tti- <tlli-* 'pay'?). - In the Mil. text 44d.44-47 (Aperlai), Xerẽi seems to indirectly reproach Xeriga for not allowing enough provisions for offerings to Trqqiz so that this latter be strong enough to guarantee peace / well-being to the inhabitants of Aperlai (as a regult, Xerẽi replaces a traditional *epeqzzi*-feast with a tribute-delivery): *χzzãtã-pe: trqqi<z> [t]rm̃mile: zm̃pde eseti: χerigazñ* 'Trqqiz finished up$^{?}$ (see *zm̃p-*) the Xeriga's allotment [for Trqqiz] for peace / well-being (d. sg. *eseti*) for Lycians (d. pl. *trm̃mile*)'; then: 'therefore, I'll replace the Aperlan *epeqzzi*-feast with a produce-delivery, then I'll tie / link it [the feast] to the next tribute-payment'. - See *trppala-* 'replace' and *kal-* 'tie / link (to)' for the subsequent passage.

asχχa- 'secure, provide' (factit. < Anat. **ass-ahh-a-*) seems to match, in part, the Ht. vb. *ašša-nu-* (caus.) 'take care of, be done with, deliver' (EDH: 216ff.) [There is no 1-sg. pret. *as-χχa* 'I made'; note that the 2-sg. imp. form *asχχa* 'provide, secure' matches the 3-sg. pres. *asχχa-ti*]. - 44d.26-27 (Xerẽi's 3rd instruction for a feast, to follow a completion of a tribute delivery): *ker[i] lẽpri-j-asχχa* 'Provide fire / heat$^{?}$ (acc. sg *lẽpri*) for the *keri*-feast!' (acc. sg. *lẽpri* to **lamp-r-* vs. d. pl. *lẽm̃pe* to **lẽ(m̃)p-* 'burn, glow'; see below; cf. syn. *k<ñ>ta* 'heat'. - 44d.43-44 (Xerẽi in Tralles): *mulẽni-pe: zppli: ẽtre-be: asχ[χat]i a[t]rala-mu<w>a* (PN?) 'A. is providing / will provide a *mulẽni*-treat for the lower ones (= troops?)'; *mulẽni* (lit. 'strengthening'$^{?}$) is structured as *tesẽni*, a treat as well. - 44c.55 *dewis: asa: muwati: zrẽtẽniz* '(Xerẽi …) invigorates (vb. *muwa-*) the *dewis* [apposition to next], the commanders, for staunchness / loyalty', or sim. (cf. 'stability'; 'standhaftigkeit') [d. sg. or pl. *asa* is unrelated to the Lyc. preverb *ese-*]).

ãpi- 'impose (a tax)' : Ht. *(a)impa-* 'weight, burden', *anda impai-* 'be burdened, depressed, beset'; cognates in Greek (HED 1/2: 14f.). - A possible cognate is the Lyc. noun *am̃mãma-* 'fine, penalty' (DLL: 3; GL: 14f.). - 55.4 (storm-god's action): *[z]aja: ãpiti [.. a]tli: pijanuwa* '(He) imposes taxes (acc. coll.) for payments to himself (d. sg. *atli*)'; for *[z]aja* cf. nomen agentis *zajala* 'tax-payer'; see sub *pssa-* 'deliver'.

da- 'put, place' (only in 55) : Lyc. *ta-* id. : Ht. *dai-/ti-* 'lay, put, place' < IE. (cf. EDH: 806ff.) [not related to Mil. *tu-* 'put, place' and inf. *tñne*; see below]. - 55.3 (a division-mark after d.-pl. *uweme* is quire clear) *me uweme: mleje: pri-pe trija* (all.) *date qir{:}zẽ* (g. pl. of *qirza-* 'share') *qabalimedi: s<ep>tãmi* (acc. sg.) *udrñte* (d.-l. pl.) 'Now, for the meal-related (adj. *mleje*, d. pl. to *mlei(je)-*, type: *murei(je)-*) libations (d. pl. *uweme*, to *uwa/e-* 'libate'), first, (he) placed the seven(-drink$^{?}$) of [various] shares, along with the bovine$^{?}$ (ins. in *-di*), at the *udrñte*-stands / trays …' (cf._*udre-_). The all. *trija* seems to refer to the storm-god as 'exhausted one' (:Ht. *tariye/a-* 'become weary', *dāriyanu-* 'make tired', to *tarai-* / *tari-*, cf. EDH: 840); but after lavish offerings this god clearly becomes very active (his activity is described in a number of passages; see strophes 55.VI-IX). - 55.4 (Trqqiz's 2nd instruction out of 4; see below, sub *kiki-* 'recite'): … *sebe da* (2-sg. imp.) *χba-ladã: t[u]wẽm[i]* (d.-l. sg.) *(e)lei* (d.-l. sg.) ... *erei[m]edi* (abl.) 'And place [the statue of] Hebat-the-wife at the *(e)lei*-source$^{?}$ for a *tuwẽmi*-treat … from the supply (abl. to *ereimi-*)!'. - Note that the sequence *dadu* in 44d.36 may be rather *-d(e) adu* (see sub *a-* 'make'). - Related nouns: Mil. *ñta-da-* '[royal] tomb' (used in l. pl., as also other designations of royal tombs) and Lyc. *ñtata-* 'burial chamber' (DLL: 45); Milyan shows *d* in all related forms, related to *da-*. - For Trqizz's 1st and 3rd instruction see *epe-* 'take'; fot the 4th see *χi-* 'offer'.

ebu- 'hamper, obstruct, block' : Lyc. *ebu-* id. (see *ebudi* 'hampers / obstracts' sub *epe-*take'). - Related nouns: Lyc.-Mil. *abura-* 'security, enforcers' (lit. 'blockers'?), possibly to Ht. *epurai-* 'besiege, dam up', with parallels in Greek, - all to IE. _*$h_1$ebhur-_, or sim. (HED 1/2: 282f.); cf. *eburẽni-*, next.

eburẽni- 'secure' (vb. of the type *trbb-ẽn-i-* 'hand over, deliver', *mur-ẽne-* 'invigorate', with an 'imperfective' suff. *-ẽne/i-* : Ht. *-anna/i-*). - Cf. Lyc.-Mil. vb. *ebu-* 'hamper, obstruct, block' and noun *e/abura-* (above). - 44d.56-57 (after Xeriga's death, Xerẽi urges 'the forceful / rich ones' to secure provisions for certain rites): *atlasi:* (acc. sg., attr. to *(e)ri-pssẽ*) *ne (e)burẽni:* (2-sg. imp.) *trm̃milijẽti: (e)ri-pssẽ:* (acc. sg.) *tm̃pewẽti* (*t. t.* = voc. sg.) 'Secure this (acc. pron. *ne*), [your] own tribute-delivery, Milyan forceful-one(s) / nobitlity$^{?}$ (voc. sg.)!' (*tm̃pewẽti* = *tẽpe*, voc. sg. in 44d.63, a semantically very similar text: cf. sub *uni-* 'know'). For the noun *(e)ri-psse-* cf. *psse-* and *zi-psse-* (both in 55); see sub *pssa-* 'deliver regularly' [there are no personal names of the type *psse*, *pssesi* in Mil. texts].

elu- 'libate, intoxicate'$^{?}$ (to *alba-* above) only in the 1-sg. pres. form *elu-wi* (archaic). - 44d.42-43 (Xerẽi in the cities of Busa and Tralles): *χñtabu-pe: kñtre: eluwi-pe: busawñn[a: a]la* (?) [3 signs are lacking] 'I'll also$^{?}$ (*-pe*) libate$^{?}$ the leader (acc. sg. *χñtabu*) during the *kñtre*-feast(s)$^{?}$ for the Busan commanders [if military] / managers' [if civilian]. - The ethnicon *busawñni-* is used as apposition to *ala-*: 'for the Busans, for the commanders' (similar: acc. pl. *dewis ... zrẽtẽniz*, see sub *muwa-* 'invigorate'). For the Mil. noun *ala/i-* '(civil) authority, (military) command' cf. CLuv. *ala/i-* 'high'.

ep(e)- 'take' : Lyc. *app-* id. : Ht. *e/app-* id. < IE. _*$h_1$ep-_ id. (LIV^2: 237); this vb. is used only in 55 (44c shows *la-* 'take'). Possibly related: *e/ap-ñt-a-* 'taking(s), booty'$^?$ [not a verb], type: *pas-ñt-a-* '(acts of) protection', *χuzr-ñt-a* 'protector(s)', all. or d. pl.; *tid-ñt-a* 'drinking vessel(s)', d. pl. - 55.1 (Piχre is threatening his vassals with a punishment for not bringing both their laborers and guards to the precinct [for a tribute-delivery]) … *kudi: s[χχa]χa: q[elei] ne (e)pdi ... sttrm̃mi: sebe: pasbã* '(And now, he hampers the essential in the law during a produce-delivery = *me (e)budi-ke: prijẽ: meri: zi-psse*) … when / if a land-tenant doesn't take both the laborers and guards to the precinct …' (for n. sg. *s[χχa]χa* 'land-tennant'$^?$ cf. acc. coll. *sχχaija* 'dues' : Luv. *sahhan-* 'payment due from land-tennants', vb. *sahhan(iya)-* 'impose feudal service', EDH: 692); for _strm̃ma-*_ (with a Luv.-type development *str-* < _*sr-_) cf. Lyc. *hrm̃ma-* 'land section', DLL: 25. - 55.4 (Trqqiz is reciting rite instructions; cf. iter. *kiki-* 'recite'): *ep(e) edes(i): qajã: wesñteli: prijãmi: [...] qrbblali* 'Take, Meal-provider (or sim.; a reference to Piχre?), [the statue of] the god Qaja of Phellos for an excellent / cherished$^?$ *q.*-treat!'; cf. _*prija-_ 'cherish, love'. - For *qrbbl-al-i* (d.-l. sg. as in rite-designations *sap-al-i*, *zb-al-i*) cf. *qrbble/i-* 'goblet' (or sim.) sub *etrqqi-*.

*erei- / **_*erije-_** 'raise, levy' : Lyc. _*eri(je)-_ 'raise' : CLuv. *ari(ya)-* id., from IE. (DLL: 16); in Milyan reflected only in nouns *erei-mi-* 'store < levy' (or sim.) and *zi-(e)reimi-* 'provision/produce-store', *(e)ri-psse-* 'provision/tribute-delivery' (cf. *pssa-* deliver regularly'; for *zi-* in *de-zi-*, *zi-psse-*, *zi-we-*, *ziti* cf. *zi(je)-* 'provide, award').

*erme- 'proclaim, announce' (or sim.) : Car. *armon* 'herald' (DLL : 115), only in d.-l. sg. or pl. *ermede* 'for/during announcement(s)$^?$', the second item in a list of rite designations [*ermede* is not a verbal form].

etrqqi- 'appropriate, use'$^?$ (type: *zr-qqi-*), - possibly to Lyc.-Mil. *atra-* 'self, person'. - 44d.1 *[.....]ed(e) m̃qrẽ: etrqqi tuwij<e>di: qrbbli: [z]ireimedi* '[For/during the … celebration$^{??}$] appropriate / use$^?$ (2-sg. imp. *etrqqi*) a ration (acc. sg. *m̃qrẽ*) from the feast-related produce-supply (abl.) for the goblet(s) (d.-l. sg. *qrbbli*)!' - An emendation *[m(e)-erme]d(e)* is not excluded, cf. d. obj. *ermede* (see_*erme-_ 'announce', above). - For the subsequent **sentence see *sla-*** 'provide, award'. - For the noun ***qrbble/i-*** cf. *qrbbl-al-i* (d.-l. sg.) sub *da-* 'put, place'; for *m̃qre-* see vb. *m̃qri(s)-* 'ration, allot'.

ewẽne 'to drink' (inf.; type: *madrane*, *tñne*) : Ht. *akuwanna* id.; see Mil. *uwa/e-* 'libate < drink'. - 44d.20 *[... n]i seketu: ewẽne zusi: zbali t[---]* 'Let him / one not damage$^?$ a [vessel]$^{??}$ for Zeus (d. sg. *zusi* to n. sg. *zuse*) to drink / for drinking during a *zbali*-rite!' (see *seke-* below); for *zusi* cf. Lyc. *zeusi*. - Offering designation *zb-al-i* (d.-l. sg.) is built as *sap-al-i* and *qrbbl-al-i* (this latter is related to *qrbble/i-* 'goblet'); for *zbali* cf. Lyc. (44a.33) *zbe-tẽ* in an offering-related context (cf. GL: 431).

ẽnẽ sla- provide, award', see sub *sla-* id.; precise vb. synonyms: *sla-* and *zi(je)-*.

*ẽ-ple- (??) 'fill' < IE. _*$h_1$en-pleh$_1$-_ id. < _*pleh$_1$-_ 'be filled' (LIV^2: 482); cf. nouns *er-ẽple/i-* 'vessel' and *zi-(e)rẽple/i* 'provision$^?$-vessel', or sim.

kal- 'tie / link (with)'$^{??}$ < IE. _*k(e)lh$_1$-_ (as in Greek *κάλο/ως* 'rope', etc.) : Ht. *kal-el-iya-* 'tie up, truss' < IE. _*klh$_1$-el-_ (EDH: 429f.; HED 4: 22). - 44d.45-46 (Xerẽi is going to replace, - vb. *trppala-* below, - an *epeqzzi*-feast with a tribute delivery, abl. *(e)ripsse-di*): … *ñt[ẽ/e]nẽ: epri-ke ziti kalu* '… and (*-ke*) further/then (*ñte* < _*anda(n)_$^?$, cf. Ht.

anda(n) ‘in addition’), I’ll tie / link this [acc. sg. *ẽ/enẽ* = the *epeqzzi*-feast] to the next produce-delivery (d. sg. *epri ... ziti*)’. - A feast of the above type traditionally follows a successful tribute-delivery (or, for that matter, bringing of trophies. **OTS**). (In our case, Xeriga’s allotment for Trqqiz seems to be insufficient: cf. sub *zm̃p-* ‘finish (up)’$^{?}$ for more detail).

kiki- (or *kiki(je)-* < **ki(je)-* ?) ‘recite, be reciting’ (iter.) : Ht. *ki-ta-* [kīda-] ‘priest-reciter’ < IE. **geHy-* ‘sing, shout’, a var. of IE. **g(^{w})eH(y)-* ‘sing, recite’ (LIV^2: 183). - 55.5 (following Trqqiz’s four offering instructions): *trqqiz: ki kikiti* ‘[It is] Trqqiz who (n. sg. *ki*) is reciting (and all gods in the precinct)’. - A coonection berween the Mil. vb. *ki-ki-* and Ht. noun *ki-ta-* was noticed by IY; thid connection re-inforces an old etymology, proposed for *ki-ta-* by Berman.

****kllei-*** ‘pay’ : Lyc. *tll(e)i-* id. (DLL: 68 and 118) is reflected in a Mil. vb. noun *kllei-me-* ‘payment, assets’ (‘tribute’ in DLL: 118) [not to Mil. *kiki-* ‘recite’; there is no noun *kille*, since 55.7 contains acc. sg. *ki* + d.-l. pl. *lle* < *lele**; see *leli-* below]. - Possibly to **ki-* : Lyc. **ti-*, *tti-* < *tlli-* ‘pay’, noun *tijala* (not quite clear); cf. **asa-*.

****km̃me-*** (?) ‘press together’ (< IE. **kem-* ‘press’, cf. LIV^2: 350). Reflected in nominal derivates (<**kom-o-*): adj. *kemije-* (<**kóm-yo-* ?) ‘swarming’$^{?}$; nouns: *km̃masa-* ‘totality, property’$^{?}$; *km̃-qe-** in d. sg. *km̃qi* ‘for raid(ing) < appropriation’$^{?}$ (compound type: *kapsa-qe-**; cf. DLL: 32, 118).

****kup-*** ‘cook, smoke’ (?) < IE. **kweh$_{\imath}$p-* ‘seethe’ (LIV^2: 374; IEW: 597). Reflected in d. pl. *kup-tt-le* ‘for the cooks / chefs’$^{?}$ (d. pl., 44d.39; type: *zaja-la* ‘payer’, *muta-la* ‘strong man’); *kup-tt-* may match Slav. **kop-ut-* ‘soot’ (cf. LIV^2: 374; IEW: 597). - See sub *pije-* ‘give, present’.

kupri- ‘favor’ (denom.?) < IE. **kūp-ro-* ‘desired one’ (IEW: 596). Nominal forms: *kupri-mi-* ‘desired, favored, chosen’; Lyc.-Mil. PN *kupr-lle-* (IEW: 596f.; DLL: 118 ‘choose, chosen’). - 44c.47-48 (gods assist Xerẽi in fighting; see also *puke-*): *seb(e) ẽnari:* (acc. sg., to CLuv. *annara/i-* ‘forceful’, CLL: 14) *kupriti: turaχssali: natri* … ‘And [the god] Natri of Turaχssa (= Lyc. *turaχssi-*) favors the Mighty one [= Xerẽi] …’ . - For the preceding sentence see *puke-* ‘rescue’; for the subsequent one see *mawa-* ‘remove, invalidate’.

la- ‘take’ [not ‘release’] : Lyc. *la-* id. : CLuv. *lā-* id. : Ht. *dā-* id., all from IE. (cf. CLL: 120). Related noun: *lajata* ‘takers’ (acc. coll; syn.: *laba*, see sub **lebe-* or **laba-* ‘grab, take’, next). - 44c.55-56 (Xerẽi visits four cities after a war): ... *ali:* (acc. sg.) *muwi* (d. sg.) *lade: epñtadi* (abl. [not a verb]) *sebe: pasbã* ‘... (he = *zrẽtẽni*-Xerẽi) took both the command (*ali*, lit. ‘high one(s)’) and the troops (*pasbã*) for an invigoration (d. sg. *muwi*; cf. vb. *muwa-*) from the booty / takings / trophies’. Mil. *a/ep-ñt-a-* probably originates from *ep(e)-* ‘take’ + suff. *-ñta-* (type: *pas-ñt-a-*, *χuzr-ñt-a-*, *udr-ñt-a-*, *tas-ñt-a-*, *tid-ñt-a-*, note also *χzz-ãt-a-* ‘allotment’ < **χzza-* ‘allot’; cf. inanim. noun *χez-m̃* ‘allotment’, - and Lyc. **padrãta-* ‘provisions’$^{?}$ (a military term). - 44d.53-55 *me χeri[ga]zñ: m̃qrẽ:* (acc. sg.) *sebe: pigasa:* (all.) *seb(e) uwedri:* (d. sg.) *urasla[:] me-ne-uwe lãte: (e)ri-psse* (l. sg.) ‘Now, they took it (acc. anim. pron. *ne* preludes *m̃qrẽ*), the Xeriga’s ration / allotment, during a tribute-delivery (d.-l. sg. *(e)ri-psse*) both for the Shining one (all. *pigasa* = *trqqñta*, all. in 55.2) and for all [gods] (d. sg. *uwedri*) for the great offering(s) (*ura-sla*)’ [the form *pigasa* is all., not acc. coll.]. Cf. all. *trija* which probably denotes the ‘tired / exhausted’ storm-god.

__*lebe-__ (type: _sebe-_) or *_laba-_ ‘grab, take’ < IE. *_le(m)bh-_ or *_labh-_ (cf. LIV^2: 411f. and fn. 1). Related nouns: _lebi-_, lit. ‘taker’, acc. coll. _laba_ (synon.: _lajata_, above); _lelebe-_ ‘booty, trophies’ (or sim.), *_lebe-we-_ id., as seen in the adj. _lbbewe-li-_ ‘booty-rich’, used as attr. to d. sg. _km̃qi_ ‘for raiding / grabbing’ (55.6; nominal suff. _-qe-_ ?), see *_km̃me-_ ‘press together’. The base _lbbewe-_ <*_lebe-we-_ is structurally comparable to _tulije-we-_ ‘gathering, assembly’ (_-we-_ < *_-war_, as per Eichner, cf. GL: 388), _zi-we-_ ‘delivery’ (in d.-l. sg. _ziwi_; cf. _sebe-_),*_slm̃me-we-_ ‘addition’ (used as a Lyc. PN).

__leli-__ ‘tell, narrate’, cf. noun _leli-_ ‘tale, narrative, speech’ (acc. & d. sg. _leli_, d. pl. _lle_ < _lele*_; type: _sse_ < _sese*_, to d. sg. _sesi_) : CLuw. _lāla/i-_ ‘tongue, gossip’ : Ht. _lāla-_ ‘tongue, speech’, from IE. (cf. EDH: 515f.) - 55.2 _ne leli-χa nere … χlusã …_ ‘I didn’t narrate to the river-deities (d. pl. _nere_ : IE. *_nerH-_ ‘dive’, LIV^2: 454) [any] quarrels’.

__*lẽ(m̃)p-__ (?) ‘burn, glow’$^?$: Ht. _lāp-_ ‘glow, flash’, _lapp-_ ‘flare up’< IE. *_leh_$_2$_p-_ with an infix in Milyan, - similar in Greek _λάμπω_ ‘shine’ (cf. EDH: 519f.). Two attested Mil. nouns reflect the above verb: (a) _lẽmpe_ (d. pl.) in a word pair _tm̃m-e lẽm̃p-e_ lit. ‘to smokes, to fires’ (_lẽm̃p-e_ may actually mean ‘to the flame-holders’) : Greek _λαμπάς_ ‘torch’; (b) _lẽpri_ ‘fire, heat’ (or sim.; acc. sg., see sub _asχχa-_ ‘secure, provide’) : Greek _λαμπρός_ ‘shining’ [scarcely _lẽm̃_ < adj. *_lẽme/i-_ + _-pe_], see sub _χba-_ ‘assign < attach’. - The d.-pl. phrase _tm̃m-e lẽm̃p-e_ in Xerẽi’s passage matches Piχre’s _pures-e_ (:Ht. _pahhur_ ‘fire’) in the original passage, authored by Piχre and later very closely imitaded by Xerẽi (see sub _χba-_).

__li(je)-__ ‘release, allow’$^?$ (type: _zi(je)-_, _pibi(je)-_) < IE.*_loh_$_1$_-éye-_ ‘leave behind, remit’ < *_leh_$_1$_-_ (LIV^2: 399) : Ht. _lai-_ ‘release’, etc. - 44d.39-40 (Xerẽi in Tralles): _mlez: ñtemlesi: mire: lide-be (a)lbijẽi: trelewñne_ ‘The altar’s libation$^?$-supervisor (n. sg. _(a)lbijẽi_) allotted / provided treats (lit. ‘meals’, acc. pl. _mlez_) for the Trallian commoners’ (appositional phrase: _mire … trelewñne_ ‘for the commoners … for the Trallians’). - For _(a)lbijẽi_ (type: _terẽi_ ‘local one’; PN _χerẽi_) cf. _alba-_ ‘libate, libation’; for adj. _ñtemle-si-_ cf. noun _ñtẽ/e-mle-_ ‘sacrificial installation, altar’$^?$ (cf. DLL: 123); the internal form of this lexeme may be ‘inside (_ñte-_) + offering / meal (_mle-_)’. - For both preceding passages of 44d.39-40 cf. _pije-_ ‘give, present’.

__luga-__ ‘burn’$^?$ (trans.) : HLuv. caus. _luha-nu-_ ‘burn’ - 44d.59-60 (Xeriga’s funeral): _me qliju: χupeliju: sebe lijeiz: dde lupeliz: ni-uwe lugãtu_ (_uwe_ = emphatic ptc.) ‘Let them not burn$^?$ the funeral outfit$^?$ (acc. sg. _qliju_), as well as (_sebe … dde_) [the statues of] the sad / mourning (acc. pl. _lupeli-z_) nymphs!’; for _lupe-li-_ (with [-up-] or [-ũb-]) cf. CLuv. noun _lu(m)pa-sti-_ ‘regret’ (CLL: 129; from IE); the statues were probably made of wood. - Forms in _-z_ are used in Milyan both as n. and acc. pl., hence acc. (sic!) pl. _lijeiz … lupeliz_ ‘sad nymphs’ here, and acc. pl. _pleliz … lijaiz_ ‘Phellian nymphs’ in 55.1 (governed by _madrane_ ‘to meet / assemble’) [D. Schürr’s implausible emendation ‘_ddelu_ (acc. sg.) _p<l>eliz_ (n. pl.)’ for _dde lupeliz_ distorts the whole picture]; cf. acc. pl. _pleliz … lijaiz_ in 55.1, governed by _madrane_ ‘meet, assemble’ (sub _madrane_, next); note _welpu-_ ‘set hope’.

__madrane__ (inf.) ‘to meet, assemble’ (a frequent denom. verbal base in _-r-_, possibly to IE *_mōd-_ ‘meet’; cf. DIER: 58; IEW: 746) + acc. pl. _plieliz … lijaiz_ ‘the Phellian nymphs’ + d. pl. _wirasaja(ja)_ ‘for treats’ (lit. ‘helpings’) + g. pl. _t-m̃qrẽ_ ‘of the rations’. - As shown by a number of passages with _m̃qr-_ (both in verbs and nouns), the rationing of treats was a serious matter, performed prior to the appropriate rite / celebration. - In

55.1, Piχre 'sets hope' (*welputi*) on his pledge to the nymphad (d.-l. sg. *lijenuwi*) in order 'to meet / assemble' (*madrane*) 'the Phelian nymphs' (acc. pl.). - Cf. *m̃qri(s)-*, *luga-*, *warasi(je)-*, *welpu-*.

mawa- 'remove, invalidate' : Ht. *mau(šš)-*, *mu-* 'fall' < IE. **meuH-* 'push away' (DIER: 57); related noun: *maw-il-i-*, lit. 'remover' (a security officer or a command), structured as *qñt-il-i-*, lit. 'runner' (> 'manager'?); cf. Lyc. *qñta* 'in charge'$^{?}$. - In the next ex., Lyc. *ti* 'who' is used for Mil. *ki* 'who' (CM agrees to this): 44c.48 '(And Natri of Turaχssa favors the Mighty one [acc. sg. *ẽnari*-Xerẽi] +) *ti mlu mawate: waχsadi: wizttasppazñ* 'who, along with [his] warriors (ins. *waχssa-di*), removed / invalidated the pledge (acc. sg. *mlu* < IE. **ET**) of (= to) Wizttasppa [= Amorges]'. - For both preceding sentences in this strophe see *puke-* 'rescue' and *kupri-* 'favor'.

mrsχχa- 'cheat' : Ht. *mars-ahh-* 'falsify' < IE. **mers-* 'confuse, forget' (DIER: 57); related noun: *mrsχχa-* 'cheater (of the tax laws)' [not 'falsehood, deceit'; see sub *tubi-* 'to force']. - 44d.12 … *me-te ne mrsχχa-ti: urtuwãz: mar[ãz]* '(If later a tax-payer [i. e., 'tax-payers'] in districts is delivering [iter. *pssa-ti*; see *pssa-*] the *sχχaija*-dues for / during the *kere*-rites,) now-there (=then?) he is not cheating (*ne mrsχχati*) the tax laws'.

murẽne- 'invigorate' (type: *qel-ẽn-e-* 'accumulate') [there is no noun *murẽn-* with an abl.-ins. *murẽnedi*; actually, only this latter form can be verbal in 44c.56-57; see text below]; related words: *mure-* 'invigoration, drinking session' in d.-l. sg. *muri* 'for a drinking-session$^{?}$; *tupleleimi*-Xeriga$^{?}$ determines for *muri* certain treats / snacks (acc. pl. *[an]az*) from the booty, ins. *lelebedi*); cf. adj. *murei(je)-* in acc. sg. *tuwi* … *murei* 'invigorating / winy$^{?}$ feast' (see sub *padre-* 'present') : Ht. *mūri-* 'cluster (of grapes)', etc. - 44c.56-57 *pidritẽni: pirli: murẽnedi: tuburiz* … 'The Provider$^{?}$ (= Trqqiz??) in Aperlai$^{?}$ invigorates / will invigorate the Tuburans …'. - The vb. *mur-ẽn-e-* seems to be synonymous to *muwa-*, next (cf. quasi-synonyms in d.-l. sg. *muri* & *nuwi*).

muwa- 'strengthen, invigorate' : CLuv. *mu-muwa-* id. < IE. **mewH-* 'abundance, power' (cf. DIER^{3}: 57). Related noun: *muwa-* 'invigoration, libation' (in d. sg. *muwi*, see sub *la-* 'take'[there is no noun *muwilade*]). - 55.6 *mlu neriu: muwa-χa: tuwẽmedi: χaba: tutasiz* (voc. pl.) 'I used to strengthen [my] pledge to the river-deities [lit.: 'river-deities' pledge', acc. sg.] by treats / libations (ins. *tuwẽmedi*) at the river$^{?}$ (*χaba*), kinsmen/offsprings!' (voc. pl. *tuta-si-z* matches *ple-li-z* 'Phellians' in 55.2, cf. voc. pl. *χbadi-z* 'Xanthians', twice used by Trqqiz in his tale: 44c.37 & 44c.49-50). - Cf. Xerẽi's use of the expression *tuwemedi* … *muwa-χã* (in 44d.51-53).

m̃qri(s)- 'ration, allot' (*-s-* is an iter. suff.; cf. *χis-*, *tus-*, *pssa-*) in 3-sg. pres. *m̃qris-ti* and 3-pl. pret. *(t-)m̃qri-s-ñte*; cf. relates nouns *m̃qre-* 'ration, allotment', *m̃qrimi-* 'allotment, ritual meal' (or sim.); both verbs and nouns may appear with *ute* (d.-l. pl.?) / *ut-* / *t-*, to *uti* = CLuv. *utti-* 'drink'. - 44d.67-68 (note a complex subj. phrase): *mire (e)kedi (i)je qñtra: ilẽnedi (i)je: t-m̃qri-s-ñte: masχχm̃* 'The commoners (n. sg. coll. *mire*) with the locals (ins. *ekedi* to Lyc.-Mil. *eki-* 'place, locale'), the (urban) authority with land-tennants (*ilẽnedi*; cf. *pije-*) have rationed / used to ration the *masχχm̃*-grant' [it was used for celebrations]. - 44d.3-4 *ali-ke mlẽ mire-ke mlẽ t-m̃qri-s-ñte: wisidi: pruwa* (d.-l. sg. or pl.; cf. **uwa-* 'see') 'Both the authority and the commoners (*ali-ke* … *mire-ke*, subj. phrase) used to ration / have rationed the meal(s) (2x acc. sg. *mlẽ*) for a drinking session$^{?}$ (d. sg. *wis-id-i*; see **wisi(je)-*)'. - 55.7-8 *ubre ñz(e)* (d. pl.

'for us'?) *abrala: ute ñneri: (a)rm̃paimi: m̃qri[s]ti: zm̃pra: qelei: punamadijedi: ãala: tuχaradi* 'At the sepulcher site (*ubre*?) in the precinct (*qelei*), for us (d. pl. *ñz(e)*) the Arma's *ñneri*-priest will regularly ration / apportion for *ute* (d. pl. of *uti-* : CLuv. *utti-* 'drink', but Mil. *ute* rather means 'for feasts') the *abrala*-treats (acc. coll.) [and] *zm̃pra*-beverage$^{?}$ (if acc. coll.) [and] *ãala*-meals / snacks (acc. coll.; see sub *χi-*) with full smoking / fumigation' (ins. phrase *punamadijedi ... tuχaradi*)'. - Such interpretation seems to be corroborated by the text 55.2-3 which contains an inventory of treats for Trqqiz (all. *trqqñta*); we find here: *[an]az* (acc. pl.) ... *χlp[p]ã* (acc. sg.) ... *(a)daz* (acc. pl.) where the central component may refer to an 'Aleppa-beverage' (cf. Lyc. [< Mil.] PN Xlppasi, to Ht. Halpassi, Halpa-ziti); the significance of the *χlp[p]a*-treat (above) seems to be underlined by a subsequent adv. *kibe* 'even'$^{?}$. - A beverage$^{?}$ *zm̃pra* may have been used for periodic commemorative feasts, dedicated to Piχre and his wife; cf. *zm̃p-* 'finish up'. - A similar situation is described in the last strophe of 44d where a [future] Lyc. ruler is being urged by Xerẽi to 'pile up' (see *χupdi-*) treats for a major feast: this seems to be one of many cases where Xerẽi rather closely imitates Piχre's narrative.

nẽnije- 'drive, send, direct' : Hitt. *nenna/i-* 'drive, ride a vehicle' : CLuv. *nanna-* 'lead'$^{?}$, all from IE. (cf. DLL: 123 and EDH: 598f.) - 44d.65-66 *χumala-de* (n. sg. + acc. sg. inanim. pron., preluding *masχχm̃*) *nẽnije-ti: masχχm̃ tije: qzze mirẽñne* [a Lyc. form, possibly an ethnicon, erroneously used for Mil. *mirẽne** ?] 'X. drives / directs (or: 'will drive / direct') this, the grant, to / for the drinking sessions$^{?}$ (d. pl. *tije*; see vb. **ti(je)-*) [and] (bovine) meals (d. pl. *qzze*) for the commoners (+*χinasi-ke: sesi: m̃qri (e)kebura seb(e)* ... 'and for the feast-related distribution(s) for / to the security / enforcers, and for …)'; OTS?.

nuni- 'announce'$^{??}$ (if to **neu-ẽni-*, type *trbb-ẽni-*, *ebur-ẽni-*, *qi-ql-ẽni-*, 'imperfective' verbs) < (?) IE. **neuH-* 'shout' (cf. LIV^2: 456f.). - 44d.58-59 *lusalija: zẽna nuni* (2-sg. imp.) *ti:* (voc. 'thou') *χruwasaz* (acc. pl.) 'Announce, thou (= offering-priest Mãmre), the offerings / provisions$^{?}$ for fiery roasting$^{?}$ (d. pl. *l. z.*)!', or sim. - For the construction type *nuni ti* 'announce, thou, (+ dir. obj. + for …)', cf. *trbbẽni ti* ... 'deliver, thou' (+ dir. obj. + for …)'; see sub *trbbẽni-* 'deliver, hand over'.

padre- 'provide, deliver, present' (related synon.: *pdura-*); cf. nouns *pidr-it-ẽni-* [pidrĩdẽni] 'provider'$^{?}$ (= Trqqiz ??), Lyc. **padrãta-* 'provision'$^{?}$. - 44c.49-51 (Trqqiz is narrating Xerẽi's martial deeds and a subsequent celebration): … *tuwi-pe (e)ne: padrete: χeriga waχsa: murei: sebe zrigali nei talã* 'Xeriga presented this (*(e)ne*), the winy$^{?}$ / invigorative (acc. sg. *murei*, adj.; see sub *mur-ẽn-e-*) *tuwi*-feast, for the guards / warriors (*waχsa*), and a special$^{?}$ (adj. *nei*, acc. sg.) *tala*-treat for the Top-fighter (= Xerẽi ?)' [d. sg. *zri-gali* < *zri-qali* 'top fighter'; cf. *qelelija* 'military' (?). As it seems, the Lyc. commander-in-chief Xerẽi brought / delivered (3-sg. pret. *χusti-te*; see *χusti-*) certain assets of his defeated arch-enemy Amorges to the ruler Xeriga who reciprocated with a feast. The noun *tala-* may be akin to CLuv. *talla-* (some container; 'vessel' in Milyan?); the whole direct-obj. phrase, governed by *padre-te* 'presented', is a 5-component chiasmic construction: *tuwi ... murei sebe ... nei talã* (acc. sg. + adj. acc. sg. + connector *sebe* + adj. acc. sg. + acc. sg.); both adjectives are of the same type: *murei(je)-* : *nei(je)-* ; cf. *mlei(je)-* : *pssei(je)-*.

pas- 'protect, rescue' : Ht. *pahš-* id. < IE. (cf. EDH: 611f.). Related nouns: d.-l. pl. *pasñte* 'for the (acts of) protection' (only in 55.6 [where *pasñte* is not a verb]); *pasba-* 'protective unit, detachment' (appears both in 55 and 44, always in acc. sg.). - DS considers *udrñte ... χis[ñ]te* being 3-pl. pret. verbal forms; I see in this strophe two 3-sg. pret. verbs *da-te* 'placed (treats for the god)' ... *χis-[t]te* 'was **offering (when heat)'**

pdura- 'bring, deliver' (cf. DLL: 124; closely related to *padre-*, above) : CLuv. *paddur** 'tray'$^{?}$ to **padd-* 'carry'$^{?}$ (CLL: 175). - 44d.17-19 *me [pd]urade: erikle-be: trqqñti: p[……..(.)]i: qi[d]ras<a>di* (2x abl.) *tiu* (acc. sg.) *ñtada χñnije* 'Now, (when / as) Herakles(-Xeriga) presented to Trqqiz a vessel / libation (*tiu*) at the burial-sites of [Xeriga's] grand-mother …' (in most cases, a designation of a royal sepulcher appears in pl.: *ñtada* (above; also *ñtete* in 55.8 ??), *ube* (always about Xeriga's tomb), *plejerese [χu]pe* (?). - Acc. sg. *tiu* matches d. pl. *tije* 'for drinking sessions' (or sim.), see sub **ti(je)-* 'drink < suck(le)'. - Cf. further *padre-* 'provide, deliver, present' and **udre-* 'bring (here).

pẽni(je)- 'drive, send' (in *pẽ<n>iu*, 1-sg. pres.-fut.; type: *nẽnije-*) : Ht. *penna- / penniye/a-* 'drive (there), send' (cf. EDH: 660, 664ff.). - 44c.60-61 (as in many other cases, Xerẽi threatens potential trouble-makers): 44c.60-61 … *pẽ<n>iu: tubedi lebi: kudi: pubrati: pere ... χuzrñtasi ... χrbblatã: trqqñtasi* 'I'll send a *lebi* (acc. sg., lit. 'taker': an agent or a team?) with punishment (ins. *tubedi*) to … where (*kudi*) one damages / desecrates [= 'intends to …' ?] the supply (acc. sg. *χrbblatã*) of the protector-Trqqiz (for [the rites] …)'. - The form *pere* may mean 'in the near future, soon' (cf. *per-epñ* 'soon(er) [or] late(r)' in a tribute-related texts; see *as-* 'make'), so that the words *kudi: pubrati: pere* may, actually, mean 'where (one) will [='intends to'?] desecrate soon Trqqiz's supply'. - A very similar (original) **text** is present in the last strophe of 55.

pibi(je)- 'give' (redupl. of *pije-*, next), used in 2-sg. imp. *pibi* in 44c

pije- 'give' : CLuv. *pī(ya)-* id. (CLL: 178); from IE.; cf. also *pibi(je)-* and *pssa-*. - 44d.37-39 (Xerẽi seems to collect contributions from land-tenants during his journey to several cities): *plejerẽ: pijeti: ilẽne: qezm̃mi* 'The *ilẽne* (rural nobility$^{?}$, n. sg. coll.) give(s) a plenty at (or 'for'$^{??}$) a *qezm̃mi*-feast'; then: *m(e) eked(i) ije: qelideli: albã-pe: kupttle: muχssa: pijelu* 'Now, let me give them / present to them (d. pl. *ije*), to the cooks / chefs (d. pl. *kupttle*), along with the rural people (ins. *eke-di*, to Lyc.-Mil. *eke/i-* 'locale, district'), a tribute-related beverage (acc. sg. *qelideli: albã*) during an *m.*-rite (or sim.)'. - For *kupttle* cf. **kup-* above; *muχssa* may originate from **muge-ssar*; for the ins. form *eke-di* cf. a subj. phrase *mire (e)ke-di qñtra ilẽnedi* '(urban)$^{?}$ commoners with the rural$^{?}$ ones, (urban) nobility with the rural one' (or sim.; see sub *m̃qri(s)-* 'ration, apportion').

pina- 'give, present, offer' : Ht. *piyanā(i)-* 'present with' (DLL: 124); related verb: *nẽnije-* 'drive, direct'. - 55.6 *kapsaqẽ: pinau: ut-m̃qrimi* (d. sg.) 'I'm giving little things for a party / entertainment' (or sim.; note nominal suff. *-qe-* in *kapsa-qe-*). - Cf. *m̃qri(s)-* 'ration, apportion'; noun *m̃qre-* 'ration'.

pre- 'gallop' (:noun *pre-* 'raid'$^{?}$) : CLuv. *para-* 'drive, chase' : Ht. *parh-* id. (EDH: 634). - 44c.51-52 (Xerẽi speakes about his numerous returns from raids / battles): *se-de keri* (d. sg.) *trisu: qñnãtbisu: prete laχadi: zrẽtẽni* 'And then the protector (= Xerẽi) was galloping thrice 12 times from the battles / raids for a *keri*' (the noun *keri-* seems to refer to a feast either after a successful raid [as in this case] or a tribute-delivery; see *χupdi-* 'pile up', about such a feast). - Note that *laχa-* 'raid, battle, assault' is not related to *ula-χa-* 'killing', or 'killer(s)' (this latter possibility is suggested by IY).

prija- 'cherish, love' (or sim.), cf. *prijãmi-* 'cherished, beloved', a participle, used as an attr. to nouns with the meaning 'detachment' (acc. coll. *prijãma ... atrala*) or 'treat, libation-rite' (d. sg. *prijãmi ... qrbblali*); both in 55 only; cf. 'West-Luv.' PN Priam. - Rather to IE. **priH/y-* 'to love' (**OTS**) than to Mil. *prije-* 'foremost'.

pssa- 'deliver' (iter.; cf. noun *psse-* 'delivery') : CLuv. iter. *pi-pissa-* 'give' (CLL: 177); cf. also *pije-* and *pibi(je)-* 'give', above. - 44d.23-24 (Xerẽi provides an instructive example of former tax-cheaters, now reformed): *[m]e-j-epñ tere kere: sχχaija* (<**sahhaniya-* ?) *kuti: pssat[i:] zajala* (subj.) … 'Now, (if) later in regions / **settlements** (l. pl. *tere*), during / for *kere*-feasts, a tax-payer delivers regularly [his] dues (then he doesn't violate / cheat the tax-related rules)'; for the latter part of our passage see *mrsχχa-* 'cheat'. - As seen in several other cases as well, a tribute-delivery took part during / before a feast in which the tax-payers participated **(OTS).**

pu- 'adjoin' (DLL : 125) : Lyc. *pu-* 'allow, admit, adjoin' (GL: 289). - 44d.5-6 (Xerẽi wants a god's statue to be moved to the ritual location): *tuli<j>elije* (adj., d. pl.) *putu trlluba* (n. sg.?) *zrppedu* (acc. sg.) 'Let him (= Trlluba?) adjoin [the statue of the god] Sarpedon to those-of-the-assembly / assembly-gods!'. - For the preceding rite instruction, uttered by of Xerẽi, see *qre-* 'fill', below. - For a moving of gods' statues to ritual locations, see *epe-* 'take' and *da-* 'put, place'.

pubra- 'damage$^?$, desecrate$^?$' (or sim.), - possibly derived from a verbal noun **puwar-*, cf. CLuv. *pūwa-* 'pound, crash' (CLL: 182; apparently borrowed into Ht.) < IE. **ph*$_2$*u-ye/o-* (cf. EDH: 684). The denominal base *pubr-a-* is structurally similar to *padr-e-* / *pdur-a-* 'provide, present' (<**padur-*), *madr-e-* 'meet, assemble', *qidr-i-* 'gallop (to / from)'. - See sub *pẽni(je)-* 'send' for the only passage with *pubrati*.

puke- 'save, rescue'; a verb with a positive meaning (as correctly recognized by Melchert, cf. DLL: 126) < IE. **bheug-* 'flee; free onself' (cf. LIV^2: 84); used with an abl. *ulaχadi* 'from killers / killing' [certainly not to *laχa-* 'assault, battle']. - 44c.46-47 (the god Natri of Kaunos rescues *zrẽtẽni*-Xerẽi from being killed during a sudden assault which seems to have started a war): *ñte-ne puketi: χbidewñni: ulaχadi: zrẽtẽni* 'Then (*ñte* < **anda*; cf. Ht.) the Kaunian [god Natri] rescues the Protector(-Xerẽi) (acc. sg. *zrẽtẽni*) from killimg / killers'. - Cf. *kupri-* 'favor' for the subsequent passage; see *zrqqi-* 'plunder / steal (from)', about the assault in question.

pzzi- 'determine' (also in Lyc.); see sub *sttẽni* '(Trqqiz) becomes angry'.

qelẽne- 'accumulate, collect, preseve' (or sim.; type: *mur-ẽn-e-*) : *qla-* id. (see there), possibly to IE. **h*$_2$*wel-*. - It seems possible that *qelẽnẽti* is not a 3-pl. pres. form but a gerund-like structure of the type 'when accumulating'; similar: *χrãti*, *uwẽti*, Lyc. *hbãti* < *sbãti* < **swanti* 'pushing'.

qidri- 'rush, gallop (to / from), race' <**qi(je)-* 'run'$^{??}$ (next), possibly based on a verbal noun **qi(je)-dri-* (type: *wije-dri-* , lit. 'messenger', see **wije-* 'send'), related to *qidrasa-* 'raiding, trophies, booty' (or sim.) and *qidrala-* id. (?). - 44c.58-59 *ki-be uwe* (emph. ptc.) *neu: psseje: qidridi: laχadi: zi-(e)rẽple* 'Is anyone [n. sg. *ki*, lit. 'who(ever)'] yet (*neu*) rashing / galloping to(ward) the provision-vessels/containers?'; for the subsequent sentence see *χra-* 'keep'.

*__qi(je)-__ 'run' (??) : CLuv. *hui(ya)-* id. < IE. *_h$_2$wey-_ id. (LIV^2: 287); see a related vb. *qidri-* above. - Verbs of the type *etr-qqi-*, *zr-qqi-* seem not to belong here.

qla- 'accumulate, preserve' (syn.: *qelẽne-*, above) may be related to *qla-* 'precinct' (or sim.) < IE. *_h$_2$wel-_ > Ht. *hul(a)-* 'wind, twist, twine', *hulaliya-* 'wind around, enwrap' etc. (HED 3: 361 f.). - 55.2-3 *ẽmu-we te: qlaχa ... trqqñta* (all.) *[a]naz χlp[p]ã kibe (a)da[z]* ... 'I have accumulated here (*te*) for Trqqiz snacks (acc. pl.), even$^?$ (*kibe*) Halpa(-wine$^?$), meals …' (or sim.). For *χlp[p]ã* cf. Lyc. PN Xlppasi : CLuv. Halpa-ssi, Halpa-ziti (to Halpa 'Aleppo'), cf. GL: 124; HtFR-NMN: 139 - The subsequent strophe describes in detail offerings and libations for the storm-god who is referred to as *trija* (all.), possibly, 'the exhausted one'; see sub *da-* 'put, place'. Different: *zri-qali qelelija* (to *_qele- 'strike'_?).

qre- 'fill up'$^?$ (?) : CLuv. *hur-* 'give liquid' : Ht. *hu(wa)rai-* 'sprinkle' <IE. *h$_2$werh$_1$-* id. (LIV: 291). - 44d.4-5 (Xerẽi gives libation instructions): *albrãna-ke mlati: trqqñtasa qretu-pe* 'And let him also$^?$ (*-pe*) fill$^?$ Trqqiz's vessel(s) in the precinct (l. sg. *mlati*)!'. - For *albrãna* (acc. coll.) cf. *alba-* 'libate' (noun: 'drink, beverage').

qtti- 'drag, pull' : Ht. *huittiya-* 'draw, pull, pluck, drag' (from IE.; cf. EDH.: 349f.). It seems, the strophe 44d.III (text: 44d.7-10) contains two contrasting sentences: the former referring to an outwardly innocent action (pulling to himself a libation-vessel), - and the latter where the character in question encounters later (*epñ*), during a special *sapali*-libation$^?$ for Arm̃pa, this enraged god, whose libation is clearly gone by that time (cf. this sentence sub *tuwe-* 'encounter < see'). - The former sentence is as follows: *albm* (acc. inanim. 'beverage' [< *albm̃* before a vowel]) *ubẽ: ti: zawa: qtti-de* (3-sg. pret.) *ziti: qñnãtba: χuzrñta χerigasa: tu[k]adrala: palaraima* '(If$^?$) a *zawa*-official$^?$ [lit. 'allotter', to *_za-_?] dragged away (*qtti-de*) for himself (refl. *ti*), during a produce-delivery (*ziti*), a beverage (/vessel) of [Xeriga's] sepulcher-site (g. pl. *ubẽ*) for libations (d. pl. *palaraima*) for the 12 statue-shaped Xeriga's protectors (all. or d. pl. *qñnãtba: χuzrñta χerigasa: tu[k]adrala*)'; the latter sentence goes as follows: '[Then] who later (*epñ*), during a *sapali*-rite at the *zppli*-stand, encountered the enraged [god] Arm̃pã?'; see *tewe-*.

sebe- 'scour'$^?$ or 'observe'$^{??}$ (cf. a noun *saba-* 'watch / watchers'$^?$; acc. coll. *sabaka* id.) : Ht. noun *_šapaš-_ (?) as reflected in *šapaš-iya-* 'to scout', *šapaš-alli-* 'scout, lookout' (cf. EDH: 725); there is no etymology for this root. - 44c.54-56 (Xerẽi's actions after a war): ... *sebedi: qirzẽ: ziwi* '(he = *zrẽtẽni*-Xerẽi …) scours$^?$ / observes$^{??}$ [4 cities] for / during a delivery (d.-l. sg. *ziwi*) of shares (g. pl. *qirzẽ*, to *qirza-* 'share, allotment')'. - For both subsequent passages see *muwa-* 'invigorate' and *la-* 'take'.

seri(je)- 'elevate' (as per DS; type: *pibi(je)-*) : *zri-* 'above, over, top' (as in *zri-qali-* 'Top fighter'$^?$, pribably an epithet of Xerẽi, see sub *padre-* 'present') : Lyc. *hri-qeri* id. < IE. *_ser-_. - 44c.64-65 (end of 44c; Xerẽi addresses Trqqiz): *trqqiz: tbisu: seri-j-ekaburã: sebe masa* 'Trqqiz, twice elevate the enforcers / security (acc. sg. *ekaburã*) and the gods (acc. coll. *masa*)!' [There is no 3-sg.-pres. verbal form *serije*; such forms lack in Mil. passages]. - We may note that Trqqiz acting and speaking usually consists of two actions or utterances.

*__sese-__ (iter.) 'distribute' (?) : CLuv. *šašša-* 'release', redupl. of *ša-* id. (:Lyc. *ha-* id.; cf. CLL: 192). Reflected only in the forms of a Mil. noun *sese/i-* 'distribution' [this is not an adj. in *-si-*]: 44d.55-56 *χumala-de nẽnijeti: masχχm̃ ... χinasi-ke: sesi: mqri* *ekebura* … 'X. drives / directs this (*-de*), the grant (acc. sg. inanim. *masχχm̃* <**mask(a)-men* ?), … for the feast-related (d. sg. *χi-na-si* to *χi-* 'sacrifice') distribution(s) (d. sg. *sesi*) to the local security / enforcers (d. sg. *ekebura* < *eke abura*, see sub *ebu-*) …'; cf. *nẽnije-* 'drive, direct'. - 44d.66 (Xerẽi is speaking): *emu: me-uwe: ãzi: sse … eim̃* ... 'Now, for me, a supply$^?$ (*ãzi*) is / has been made for distribution(s) (to the protective guards)'; see sub **ai-* 'make'. - For *sse* < *sese** (d. pl.) vs. *sesi* (d. sg.) 'distribution' cf. *lle* < *lele** (d. pl.) vs. *leli* (d. sg.; also acc. sg.) 'narrative, speech, (words on the) stele'; see *leli-* 'narrate'.

sla- and *ẽnẽ sla-* 'award < provide' (cf. *slãma-* 'add', DLL: 00) <IE. **selh*$_1$*-* 'take' (LIV^2: 529): a passage with *ẽnẽ sla-* 'award' in 55.5 (awarding Piχre's personal detachment for their deeds) closely matches that with *zi(je)-* 'award' in 44d.49-50 (Xerẽi awards his detachment for martial valors). - 44c.32-33 (Natri awards the leader [= Xerẽi ?] and his detachment, apparently after a successful raid): … *sebe: pasbã[:] natri: slati: χusttedi: sebe χñtabu* '(At the monument …) Natri awards with *χustte-* (ins.) both the detachment and the leader / commander (for shares for the provision$^?$-storage(s))'; cf. a related noun: *ura-sla* 'great offering' (cf. DLL: 133); cf. also *slãma-* 'add, increase', next.

slãma- (also *slama-*) 'add (smth. to), increase (smth. in)', to *sla-* 'award, provide' (above); the verb *slãma-* is closely related to the Lyc. noun *hlm̃mi-* 'addition, gain' (DLL: 24). - 44d.26 (Xerẽi's 2nd feast instruction for a traditional celebration 'for Lycian men' after a competed tribute-delivery): *qrbbli: me-ije (a)lbãma: pssesi: slama* 'Increase the *albãma*-beverage in the tribute-related goblet(s)!'; cf. *χupdi-* 'pile up (treats)' for the 1st instruction, and *asχχa-* 'secure, provide (heat)' for the 3rd one; cf. also *qrbble/i-* 'goblet, drinking-vessel'.

sm̃me/a- 'oblige' (?) : Lyc. *sm̃ma-* 'bind, enjoin' (DLL: 58). In Milyan this base is only reflected in a d.-l. pl. phrase *sm̃mẽte: klleime* 'to / for obligatory (lit. 'oblyging', participle) payments / contributions' (44d.60-61) [Lyc.-Mil. *sm̃m-* cannot match *zm̃-*].

stt[ẽ]ni (3-sg. pres. mid.) 'becomes angry', - about Trqqiz ('and all gods in the precinct', 44d.13-16); to Lyc. *httẽmi-* 'anger, wrath' (DLL: 26 and 129). - Trqqiz becomes angry because a sacrifider (*[k]em(i)* ?) doesn't determine yet (*neu* ... *pzziti*) a tasty victim (*ekãnẽ kuprimi*) for Zina/Zeus (d. sg. *zini* = *zusi* = Lyc. *zeusi*).

te-tbe- 'break (an object)' [*tetbeti* is not a noun; *apñtadi* is not a verb] : Lyc. *tebe-* 'destroy (an army)'. - 44d.33-34 *nike dezi: mutala: apñtadi: tetbeti* 'And [there shall be] no additional provision (n. sg. *de-zi*) [because] a(ny) strong man (n. sg. *mutala*) will / may break the *laγra*-stands with the booty / takings' (ins., as in *ñtuwitẽni waχssadi* 'commander with the warriors'). - Cf. abl. *ep-ñta-di* 'from the booty / takings' sub *la-* 'take'. - For *dezi-* cf. *zi-* in compounds (see sub *zi(je)-* 'provide, award').

tewe- 'enounter < see' (?) (cf. DLL: 130) : Lyc. **tewe-* 'eye, sight'. - The strophe 44d.7-10 (of which the 1st sentence is analyzed sub *qtti-* 'drag, pull') seems to read as follows: (1) '(If$^?$) a *zawa*-official [lit. 'allotter', to **za-*?] dragged away (*qtti-de*), during a produce-delivery, a beverage (/vessel) of [Xeriga's] sepulcher-site (g. pl. *ubẽ*) for libations (d. pl. *palaraima*) for the 12 statue-shaped Xeriga's protectors, (2) [Then] who [else] later (*epñ*), during a *sapali*-rite at the *zppli*-stand, encountered the enraged [god] Arm̃pã?'; acc. sg. *arm̃pã* may denote Trqqiz or another Lyc. god, Arma. - This seems to be one of several cases where Xerẽi uses rhetorical questions.

ti(je)- 'drink < suck(le)' (type: *zi(je)-*, *pi(je)-*, *li(je)-*) : Lyc. *tidei-* (in *tidei-mi-* 'child, son' < 'nurtured') : Cluv. **tītai-* (in *tītaimmi-* 'nurturing'; CLL: 228) < IE. **dheh$_1$ye-* < **dheh$_1$i-* 'suck(le)' (LIV: 138; EDH: 875). - Cf. related words for 'drink, vessel': acc. sg. *tiu* (+ *trqqñti* 'for Trqqiz'; see sub *pdura-* 'present'); d. pl. *tije* (+ d. pl. *mirẽnne* < *mirene** 'for commoners'; see sub *nẽnije-*); *tidñta* 'to the vessels' ('women' per DS) : Ht. *tītanta-* (a particple, as per Tischler). - Cf. *χba-* 'assign'.

tirbe- 'remove, purge' or 'smash', possibly to IE. **terbh-* as in Slav. **terbiti* 'purge, stub', etc. (cf. HGE: 430) [less likely to **dherb-* 'hit', LIV^2: 153]. A related noun: Mil. *terble-* 'deletion' or 'damage' (+ d.-l. pl. *lle* < *lele* 'to/at the inscription / words', to noun *leli-* 'tale' and vb. *leli-* 'speak, narrate', above). - 55.2 (Piχre warns potential tax-cheaters): *eke: pleliz: abura: me (e)bei: tirbeti: zirãpla* 'Now (*me*), Phellians (voc. pl. *pleliz*), the security / enforcers (n. sg. *abura*) in locales (l. pl. *eke*) will purge / smash him (? d. *ebei* may refer to any potential cheater) the produce-vessels'; acc. coll. *zirãpla* matches d. pl. *zi(-e)rẽple*, *erẽple* 'to the vessels'. - As it seems, the vessels are to be confiscated or, for that matter, smashed. - For more detail cf. *ebu-* 'hamper, obstruct'; for *zi-* see *zi(je)-* 'provide'.

tñne (inf.) 'to place; pay (fine)'; see *tu-* 'put, place' (below) : Lyc. *tuwe-* 'place upright' : Lyc.-Mil. iter. *tu-s-* < IE. **(s)teh$_2$w-* id. (DLL: 74 [thus not related to Mil. *da-* & Lyc. *ta-* 'put, place']). - 44d.10 *atli tñne: qã[.]ã* '(Trqqiz forces / will force a tax cheater) to pay fine$^?$ to himself (= Trqqiz)'.

trbb- 'arrange; hand over'$^?$ (cf. DLL: 131) : CLuv. *tarawi(ya)-* 'hand over' (cf. CLL: 211). - 44c.37 (according to H. Eichner, Trqqiz starts here his narrative (using voc. pl. *χbadiz* 'Xanthians'): *laγra: trbbdi: χeriga: me χbadiz: kudi ...* 'Now (*me*), Xanthians , Xeriga arranges / is arranging the *laγra*-stands, as (he) …' (cf. about this passage also sub *uwa/e-* 'libate < drink'). - 44d.27 (preparation for a feast): *muni: trbbdi tasñtu (u)wadi ...* (word division according to DS) 'Muni (if PN) arranges / habds over a *tasñta*-stand (:Lyc. *tahñta*) with bovine(s) …'. - Cf. related vb. *trbbẽni-* 'deliver, hand over', next.

trbbẽni- 'hand over, deliver' (cf. DLL: 131 [but there is no 3-sg. *trbbẽni-ti*]; cf. syn. *trbb-*). - 44d64-65 (Xerẽi urges 'the forceful ones / nobility' to deliver their dues to be used at the *asãnamla*-ritual): *trbbẽni* (2-sg. imp.) *ti* (voc. 'thou') *ne kñmẽti: punamadedi: asãnamla* (d.-l. sg.) 'Deliver, thou, this (*ne*), everything/all (*kñmẽti*), for/at the blood-sacrifice!' (a major rite). - Cf. the preceding passage sub *uni-* 'know'. - For *trbbẽni ti* 'deliver, thou,' (+ direct obj. + 'for …') cf. *nuni ti* 'announce, thou', (+ direct obj. + 'for …'); see sub *nuni-* 'announce'$^?$.

trppala- 'replace', noun *trppali-* 'replacement' (='2nd helping' during a feast in Xanthos; with 2-sg. imp. *tu* 'place') : CLuv. noun: *tarpalla/i-* 'ritual substitute' (DLL: 131; CLL: 214). - 44d.45-46 *epe-qzz[i] trppalau: (e)ripssedi ...* 'I am replacing (or: 'will replace') an *e.*-feast (acc. sg.) with a tribute-delivery …'; see *kal-* 'tie to' in the subsequent text; 44d.21-22 *tbisu tustti arm̃paimedi: qã[.adi .]utla-de: ñte ... terẽi: ki tewẽm̃ tunewñn[i]* 'A local will twice pay any (= 'what(ever)', *ki*) equivalent$^{??}$ (or sim.) to *tunewñni* (= Trqqiz), along with the fines to Arma (lit: 'along with Arma's fines', ins.)'.

tu- 'put, place' (cf. inf. *tñne* above) : Lyc. *tuwe-* 'place upright' < IE. **steuh$_2^w$-* : Lyc.-Mil. iter. *tus-* 'pay (fine / tribute)', with a dative obj. which refers to gods. - 44d.58 (Xerẽi urges an offering priest to present some *medu*-beverage to the enforcers who help with the offering preparation): *sẽkẽne: mãmre (e)kebure: medu tu* 'Place wine (*medu* : CLuv. *maddu*), [priest] Mãmre (voc.), for the security / enforcers (d. pl.) for keeping fire (?)!' [acc. sg. inanim. *medu* (:CLuv. *maddu* 'wine') doesn't reqire an emendation *me-d<e>-*]. - 44d.34-35 (Xerẽi gives instructions to functionaries in connection with a feast in Xanthos): *me tu-pe-ne tesẽni: qñza: prijelija: me-de tu χezm̃ χbadasa* 'Now, place this (acc. sg. anim. pron. *ne*), the *tesẽni*-dish, for the (bovine) meals (d. pl. *qñza*) for the nobility (noun or adj. *prijelija*); now put it (inanimate pron. *-de*), the allotment (inanim. noun *χezm̃*), (for meals) for the Xanthians …'.

tubi- 'force' (+ direct obj. + inf.) : Lyc. *tub(e)i-* 'strike' : CLuv. *dūp(a)i-* id. (DLL: 72; from IE.). - 44d.10-11 (Xerẽi is threatening potential tax dodgers) *atli tñne: qã[.]ã: pri-j-eduli se: trm̃mile: kupr[l]lese me-pe-ne tubidi: urtu: mrsχχã: trqqiz … lusasi: esẽnẽmla* (d.-l. sg.) 'To place / pay (inf. *tñne*) a fine$^?$ (acc. sg. *qã[.]ã*), first (*pri*) to himself (*atli*), and [then] to the Lycians of Kupprlle [= royal Lycians], for damage (d. sg. *edul-i* : CLuv. *adduwal-* 'evil') … Trqqiz forces / will force a tax (adj. *urtu*) cheater … during a fiery blood-offering'; cf. inf. *tñne*. - Mil. *eduli* is not influenced by the Čop's Law (similar: acc. sg. inanim. *medu* : CLuv. *maddu* 'wine'). - Thid is one of several cases which show that both Piχre and Xerẽi would punish tax-law violators with forcing them to pay the dues - usually, a double amount (*tbisu* 'twice'; see sub *trppala-* 'replace').

tutl- (< redupl. **tu-tul-* ?) 'multiply / magnify'$^?$ (+ acc. obj.) at the very end of 44d [DS transcribes *χñtaba tutltu-[.]e* (subj. + 3-sg. imp. + ptc.); there is definitely no vb. *χñtabatu*]. In the next-to-last strophe of 44, Xerẽi urges any [future] Lyc. ruler to arrange annual (?) commemorative feasts (see sub *χupdi-*); the beginning of the last strophe, *neiz-ke: tuwiz trm̃mile* 'and the special$^?$ feasts for Lycians', continues Xerẽi's utterance with *χupdidu* 'let him pile up (treats)': 44d.70-71 *neiz-ke: tuwiz trm̃mile: sukri: χñtaba tutltu-[p]e: trqqñti* [not finished] 'And let the leader / ruler (n. sg. *χñtaba*) multiply / magnify (or sim.) special$^?$ feasts for Lycians [and ?] libation(s) (acc. sg. *sukri* ?) for Trqqiz …'; or: '… during libation(s) (d.-l. sg. *sukri*) for Trqqiz …'; note ins. *sukredi* 'with libations' in 44d.50 [this is not a verb; cf. *sla-* 'provide, award']; we may see that the treats *neiz tuwiz* (acc. pl.) and *nei talã* (acc. sg.) are provided by a ruler of Lycia (*χñtaba*; *χeriga*); this may imply that *nei(je)-* (which we have interpreted as 'special') may, actually, mean 'royal', - cf. Ht. *nēya-* (etc.) 'lead'.

****udre-*** 'bring (here)' is reflected only in d.-l. pl. *udr-ñt-e* 'to/at the *u.*-stands' (type: *pas-ñt-e* 'for the [acts of] protection) in the offering description 55.3; cf. Ht. *u-da-* 'bring (here)' (EDH: 931). There exists an opposition Mil. **udre-* 'bring (here)' : Ht. *u-da-* 'bring (here)' vs. Mil. *padre-* 'bring, present (a treat for)' : Ht. *pe-da-* 'take, carry' (cf. EDH: 666 & 673). Mil. *padre-* / *pdura-* 'present' originates from **padur*, a pendant to CLuv. **paddur* 'tray' (ultimately a borrowing from Ht.) - See sub *padre-*.

uni- 'know' : CLuv. *uni-* / *unai-* id. : HLuv. *uni-* id. (CLL: 241); a genetic link to **u-* 'see' (cf. **uwa-*, next) is not excluded. - 44d.63 (soon after the funeral of Xeriga, the new ruler Xerẽi urges the nobility to deliver their dues in full): *uni tẽpe: urtuz: marãz* 'Know (2-sg. imp.), nobleman / forceful one (voc. *tẽpe*), the tax-related laws / rules!'; see the subsequent part of this strophe sub *trbbẽni-* 'deliver, hand over'. - For *tẽpe-* cf.

syn. *tm̃pewẽti-* in 44c.58 (see sub *murẽne-* 'invigorate') and 44d.57 (sub *eburẽni-* 'secure').

***uwa-** 'see' : Ht. *au-/ū-* 'see' < IE. **$h_1$ew-* id. (LIV^2: 243); possibly reflected in the emphatic particle *uwe* (< 2-sg. imp. 'look!' ?) and in the d.-l. form *pr-uwa* 'under observation' (or sim.; about controlling a major celebration = 'drinking session'; see **wisi(je)-*) : Ht. *parā-uwa-nt-* 'supervisor' (EDH: 634). Mil. *pruwa-* is seen in the adj. *pru-χ-ssi-* (< factit. **pruwa-χa-* ?), attr. to *epe-qzzi* , a feast for men; see**qãz-*. There seems to be no other reflections of IE. **$h_1$ew-* 'see' in Milyan; the verb *uwa/e-* means not 'see' but 'libate < drink' (see below; cf. *uwemi-* 'libation' and *uti-* 'drink').

uwa/e- 'libate < drink' (:verbal noun *uwemi-*, cf. HLuv. *uwami-* 'having drunk') : HLuv. **uwa-* 'drink' : CLuv. *u-* id., note Mil. *uti-* 'drink' (in d. pl. *ute*) : CLuv. *utti-* 'drink'; note Mil. inf. *ewẽne* 'to drink' (above) : Ht. inf. *akuwanna*; all to IE. *h_1eg^wh-* 'drink' > Ht. *eku-* id. (cf. LIV^2: 231). - 44d.47-48 (Xeriga in Antiphellos; the city name may have been Wzzaije) *[w]esãtñniu: qñtbẽ uwaχa: mlati: wzza{:}ijesi* 'I have libated the Phellian [god] Qñtbe in the precinct (l. sg. *mlati*) of Wzzaije/Antiphellos'; cf. voc. *wzzaijesi* '(Man) of Wzzaije' in 55.5 ; see sub *kiki-* 'recite').

***warasi(je)- / *wirasa-** < **warisa-*$^?$ '(come to) help' (iter.) : Ht. *warrissa-* id. (cf. HLuv. *wariya-* 'to help). Cf. Mil. nouns *wirasaja(ja)* (see sub *madrane* above) and *warasijez*. - Related: Mil. *weri-* 'helper, superwiser' : Ht. *warri-* / *warrai-* 'help(ful)' < IE. **worH-i-* (EDH: 962). - The form *warasijez* seems to be voc pl. (type: *tutasiz*, *pleliz*, *χbadiz*); approx. meaning: 'helpers; combattants', or sim.

welpu- 'set hope (on)' [most certainly, a favorable action; participle *welpumi-* 'trustworthy, reliable'] < IE. **welp-* id.; used with a direct obj. *[eb]añn[ã] mlu* '(Piχre sets hope on) this pledge (to the nymphad)' in 55.1. - For *welpumi* see *zi(je)-* 'award < provide'.

***wije-** 'send'$^?$: Ht. *wije-*, *uye-* 'send (here)' (preverb *u-* + vb.; cf. EDH: 909ff. & 1012). Reflected only in the noun *wije-dri-* '(low-rank) commander / (civil) authority', possibly originally 'messenger'. Cf. *tuke-dri-* 'statue', etc.

***wisi(je)-** 'press' : CLuv. *wiši-* / *wišai-* id. (CLL: 270). - Mil. *wisiu* and *wisidi* are nominal, not verbal, forms: a 3-pl. pret. vb. *(t-)m̃qris-ñte* precedes the form *wisidi* 'for a drinking / beer$^?$ session'$^?$ (d.-l. sg. with a suff. *-id-*) in 44d.3; a direct-obj. phrase *ki wisiu* 'any / whatever *wisije*-drink$^?$' precedes an imp. form *tu* 'place (as a treat)' in 44c.56 (note here d. pl. *ute*, to *uti-* 'drink' : CLuv. *utti-* id.); cf. imp. *tu* in 44d.58: *mãmre (e)kebure: medu tu* 'Mãmre (= PN in voc.), place a *medu*-beverage (:CLuv. *maddu*) for the enforcers!' - See sub *tu-*.

χba- 'assign < attach' : Lyc. *χba-* 'inflict < attach' : CLuv. *hap(a)i-* 'attach' (CLL: 55; lenition of **happ(a)i-*). - 44c.34-35 *trqqiz ... χi* (d.-l. sg.) *χbati: qetbeleimis* ... 'Trqqiz assigns to the offering[-preparation$^?$] the *q.*-gards'. - 55.4 *kuli-ke: mru[w]asi: tidñta: χbade* '(he = storm-god) assigned / used to assign the patrol$^?$ (acc. sg. *kuli*) of the stele to the vessels' (for *tidñta* cf. **ti(je)-* above) - Similar in 44c.63-64: *wiχsaba laba me ... χbade ... tunewñni: seb(e) erẽpli: sabaka qetbeleima* (acc. coll.) 'Tunewñni(-Trqqiz) assigned / used to assign the warriors-takers$^?$ (appositional phrase *w. l.*, acc. coll.) to ..., and (he assigned) the *s. q.* (acc. coll.) to the vessel(s) (d. sg. *erẽpli*; see **ẽ-ple-* 'fill')'.

χi- 'sacrifice' ('make an animal sacrifice', DLL: 83 & 135), iter. *χis-* : noun *χi-* '(bovine) offering, sacrifice', possibly to Lyc. nouns *aχa-* 'animal sacrifice', *aχãti-* 'priest of animal sacrifice', *aχãta-za-* id. (DLL: 7); see sub *χba-* 'assign' < 'attach', above); note adj. *χi-na-si-* 'pertaining to offering/feast' [not to *χñna-* 'grand-mother'; see sub *nẽnije-* 'drive, direct'], and possibly *χapa-* (d.-l. sg. in 000). - 55.4 (Trqqiz's 3rd offerins instruction, probably assressed to to Piχre): *ãla: χi: zinase* 'Sacrifice (2-sg. imp.) an *ãla*-offering (< acc. coll. _*anala_, as per DS) to Those-of-Zina(/Zeus)!'

χra- 'keep' [not 'offer', not to *χruwasaz* 'offerings'] as in *mlu χrau* 'I keep a pledge (...)' (both in 55 and 44d); the closest match seems to be Ht. *har(k)-* 'hold, have, keep', *har-want-* 'keeper' (EDH: 304f.). - Note Xerẽi's warning to potential raiders (44c.59-60): ... *erẽple:* (d.-l. pl.) *χradi:* (3-sg. pres.) *waχsa:* (acc. pl. or coll.) *truijele: m(e) ẽmi: mawili* 'Now, at the vessels /containers, my enforcer(s) keep(s) the *waχsa*-guards during the *t.*-celebrations (d.-l. pl.)'; cf. d. sg. *trujeli* in 44c.33-34: *natri ... laγra: trujeli: zazati* '[God] Natri ... is arranging the *l.*-stands (:Ht. *lah(h)ura-*) for a *trujeli*-celebration'. - For *erẽple* cf. d. pl *zirẽple* < *zi-(e)rẽple* sub *qidri-* 'rush, race', and acc. coll. *zirãpla* sub *tirbe-* 'smash'.

χupdi- 'pile up (treats)' < noun _*χup-id-_ 'piling-up'$^?$ (type: *wis-id-*, *qel-id-e-*, _*mrγγ-id-_ in d.-l. sg. *mrγγd-i*) : Ht. *hu(wa)pp-* 'hurl, throw (+ acc.)', noun *hūppa-* 'heap' (EDH: 369). - 44d.25 (Xerẽi's instructions for a traditional feast after a successful tribute-delivery): *trm̃mile-be te keri: trei χali pise: χup[di]* 'For the Lycian men (d. pl. *trm̃mile ... pise*), pile up here a *keri*-feast for three portions!' (d.-l. sg. *trei χali* : Ht. *hali-* 'ration, portion, share', cf. HED 3: 23f.). Mil. d. pl. *pise* seems to presuppose n. sg. _pisẽ*_ 'man', cf. Ht. *pišen-* 'man / mail'; Mil. n. sg. _pisẽ*_ structurally matches n. dg. _tssẽ*_ (to d.-l. pl. *tsse* in Xerei's feast instructions); cf. Lyc. n. sg. *θθẽ* vs. Mil. d.-l. pl. *tsse*. - Note that an emendation *χup[:)]* instead of *χup[di]* (as above) is not acceptable because the passage with 3-sg. imp *χupdi-du*. 'let him pile up (treats)' shows a structure which is very similar to that in the passage with *χup[di]* (above): in both texts, the expression *trei χali* 'for three portions' is used, though in the text with *χupdi-du* a form *trisu* 'thrice' is added (this text refers to a much larger feast).

χusti- 'rush / quickly deliver (smth. to)$^?$' (:noun *χustte/i-* 'dodging, evasiveness'$^?$ [an important skill in fighting]) : (?) Ht. *hu(e)sa-* 'spindle' < IE. _*h$_2$weys-_ 'wind, twist' (:noun _*h$_2$woyso-_, cf. HED 3: 341ff.; note Russian *вихрь* 'whirlwind', etc.) - An alternative etymology may be based on the HLuv. vb. _*hwi-s-_ 'run' < IE. _*h$_2$wey-_ id. (LIV^2: 287); cf. vb. *qidri-* above. - In the strophe 44d.49-51, Trqqiz (using voc. pl. *χbadiz* 'Xanthians') narrates Xerẽi's rushing to the Lyc. ruler Xeriga the assets of the defeated Umrgga / Amorges: ... *χustite: umrggazñ: klleimedi: sbirtẽ: χbadiz* '(As he) rushed [='has delivered'?] Umrgga's share / assets with contribution, Xanthians, ...'. - In the subsequent sentence, Xeriga presents a feast both to the warriors (*waχsa*) and to their commander Xerẽi (*zri-gali* < *zri-qali* 'Top fighter'): see *padre-* 'present'.

__*χzza-__ 'allot, ration' : CLuv. *hizza(i)-* 'hand over' (CLL: 70) : Ht. *hink-* (etc.) 'bestow, offer' (EDH: 268ff.) < IE. _*h$_2$eink̂-_ id. (cf. EDH: 270 and LIV^2: 268). Cf. related nouns in: 44d.44 *χzzãtã ... χerigazñ* (acc. sg. anim.) 'Xeriga's allotment' (used for offerings to Trqqiz); 44d.36 *(e)de ... χezm̃* 'this, the portion/ration' (acc. inanim., as seen in the pron. *(e)de*); *χez-m̃* is an inanim. noun with a suff. *-m̃* < *-men,* as also in *masχχ-m̃* 'grant', *alb-m̃* (*albm* before a vowel) 'beverage' (see *alba-* 'libate' and *tu-* 'put, place (as a treat)'. - Cf. *zm̃p-* 'finish up' for the passage with *χzzãtã*, and *trppala-* 'replace' for that with *χezm̃*.

zaza- (iter.) 'arrange' (+ direct obj. *layra* '*l.*-stands'; *tuburiz* 'tuburans', Xerẽi's personal guards); 'allot, distribute'$^{?}$ in DLL: 137. Evidently, a reduplication of **za-* 'allot, deliver'$^{?}$: Lyc. noon *za-* 'allotment, portion' (DLL: 87) : Lyc. *ze-* 'put down (a body)' [= 'bury' / 'kill'; cf. *ẽti zehi* 'in a fight'] (*ze-* 'assign a share [= place in the tomb] to'$^{?}$ in DLL: 88). The Lyc. vb. *dde-ze-* 'set aside'$^{?}$ (or sim.; DLL: 10) genetically matches the Mil. noun *de-zi-* 'additional delivery'$^{?}$ (DLL: 10 and 114 [*dezi* is n. sg., not d.-l. sg.]), cf. a nominal component *zi-* 'provision, contribution'$^{?}$ (as in *zi-psse-* 'produce / tribute delivery'; see vb. *pssa-* 'deliver'), noun *ziti* (d.-l. sg.) 'produce delivery'$^{?}$ (cf. Lyc. *u[h]a-ziti*, *uhahi [z]iti*, DLL: 76, vs. *uha-zata* 'yearly tribute' to *zata* 'tribute', ibid.: 75); note Mil. acc. coll. *[z]aja* (or *[z]ata* ?) 'taxes / tribute', n. sg. *zajala* 'taxpayer' (see sub *ãpi-* 'impose'). - As it seems, *za-*, *ze-*, *zi-* show an iter. suff. **-sk̂-*, thus being geneticall related to Mil. *da-* & Lyc. *ta-* 'put, place' (cf. DLL: 87). - For the above *zi-* 'provision'$^{?}$ cf. Mil. *zije-* 'provide > award' (syn. to *sla-*). - 44c.32-34 *natri ... layra: trujeli: zazati* 'Natri arranges the *layra*-stands (acc. coll.; cf. Ht. *lah(h)ura-*) for a *trujeli*-celebtation' (vs. d.-l. pl. *truijele* in 44c.59; see *χra-* 'keep'). - 44d29-32 (a feast is interrupted by arriving warriors) ... *ñtuwitẽni: uplesiz: waχssadi: tubu<r>iz ẽke-d(e) epñ: predi: zazati: zri-qali* '(And there shall be no entertainment) when later the commander-in-chief, the Top fighter [both nouns denote Xerẽi], along with the guards (*waχssadi*), arranges the noble$^{??}$ (acc. pl. *uplesiz*) Tuburans [coming] from a raid / raids'. Note a frame construction *ñtuwitẽni ... zri-qali* 'the commander-in- chief … the Top fighter'; similar: *qñtili ... tunewñni* (d. sg. in 44d.61-62) 'to the Overseer (or sim.) … to Tunewñni (= Trqqiz?)'; for *qñtili* cf. DN Qñtbe (= Lyc. PN) and Lyc. *qñta ti* 'who is in charge' (or sim.).

zi(je)- 'provide, award' (a precise synonym to *sla-* / *ẽnẽ-sla-*), cf. *zi-psse-* 'provision / produce delivery' and *zaza-* 'arrange' (above). The vb. *zi(je)-* (type: *li(je)-*, *pi(je)-*) is represented by a 1st-sg. form *ziu* (see ex. below). - For *zi(je)-* cf. a verbal noun *zi-we-* 'contibuting, contribution' (type: *lbbe-we-** to **lebe-* 'grab, take'), see sub *sebe-* 'scour'$^{?}$. - 44d.48-50 (Xerẽi visits Piχre's sepulcher in Wzzaije/Antiphellos$^{?}$): *me welpumi: mrγ[γ]di pttili-ke: χustti-ke qidrala: ke-pe-n[e] ziu: sukredi: kibe: pasbu* 'Now, at the *m.*-location, I'm awarding / I'll award (*ziu*) with *sukre-* [my] trustworthy detachment (acc. sg. *welpumi ... pasbu*) for evasiveness, and for agility, and for procurement' (or sim.); cf. vb. *χusti-* for thr noun *χustti*. - Note that *welpumi ... pasbu* (acc. sg.) matches semantically the phrase *prijãma ... atrala* 'cherished detachment' (acc. coll.) in a very similar strophe 55.5-6; as in many other cases, Piχre's original passage was later imitated by Xerẽi.

zm̃p- 'finish up'$^{??}$ (subj. *trqqiz*): 44d.44-46 *χzzãtã-pe: trqqi<z> [t]rm̃mile: zm̃pde* (3-sg. pret.) *eseti: χerigazñ* 'Trqqiz finished up / consumed (?) Xerigas's allotment (acc. sg. *χ. ... χ.*) for [the sake of] the piece / well-being for (= of) the Lycians …'. - Xerẽi seems to reproach, indirectly, his predecessor Xeriga for not allotting enough provisions for important offerings; in the subsequent passage, Xerẽi sais: 'therefore, I am replacing (or: 'will replace') the *epeqzzi*-feast with a tribute-delivery …' (see sub *trppala-* 'replace' and *kal-* 'tie / link'); normally, such feast follows a successful tribute-delivery. - For the vb. *zm̃p-* cf. noun *zm̃pra* (sub *m̃qri(s)-* 'ration').

zrqqi- 'plunder / steal (from)' (type: *etr-qqi-*, with a verbal suff.) < (?) IE. **ser-* 'take, grab' (cf. LIV^{2}: 535); cf. a related noun *zrbbla-* 'booty'$^{?}$ < IE./Anat.**sorw-* (as in MIr. *serb* 'robbery'; see sub *slãma-* 'add, increase') : Ht. *šāru-* 'booty, plunder' (EDH: 738f.). - 44c.44-45 (Xerẽi describes a sudden attack of an invader which seems to start a war) : *me uwe kemijedi: waχsadi: zrqqiti zireime{me}di: χbadasadi* 'Now look! With swarming? warriors (he = the invader ?) is plundering / stealing from the Xanthian supply-stores ...'; *uwe* may retain the underlying meaning 'look!'; cf. **uwa-* 'see'. - This passage contains a 5-component chiasmic structure: *kemije-di: waχsa-di: zrqqi-ti zireime-di: χbadasa-di* = adj. in abl.-ins. + noun in abl.-ins. + vb. + noun in abl.-ins. + adj. in abl.-ins. *zrqqiti*; note sound-ornamentation: *-e-di* + *-a-di* +*-i-ti* +*-e-di* + *-a-di*.

References

CLL = Melchert H.C., *Cuneiform Luvian Lexicon*, Chapel Hill, N.C., 1993.
CM = Melchert H.C.[1]
DLL = Melchert H.C., *A Dictionary of the Lycian Language*, Beech Stave Press, Ann Arbor - New York, 2004.
DS = Schürr D.[1]
EIW = Pokorny J., *Etymologisches indogermanisches Wörterbuch*, 1. Bd., Francke, Bern - München, 1959

---

[1] Reference to correspondence or oral discussion.

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

Global Etymologies and Alfredo Trombetti

Shamil Nafiqoff
Russian Academy of Sciences[*]

Abstract

The article offered presents a brief outline of the contribution the famous Italian macro-comparativist Alfredo Trombetti has made in the field of the so-called Global *etyma*, being among the first who studied and practiced this approach at the turn of the XIX-XXth centuries. A number of *comparanda* are demonstrated to be research subjects of the subsequent long-range linguistic scholars with particular instances presented.

Foreword.

It is common knowledge that the great Italian comparativist A. Trombetti is often referred to as 'father' of long-range research, and of the so-called 'global etymologies' in particular. He was a predecessor or precursor of such noted long-range linguists as Vladislav Illich-Svitych, Morris Swadesh, Aharon Dolgopolsky, Joseph Greenberg, John Bengtson, Merritt Ruhlen, Vitaly Shevoroshkin, Sergei Starostin, to name but a few. Many of the above mentioned scholars have cited Trombetti in their sources or references. This is the case with Illich-Svitych 1971, Swadesh 1960, Bengtson and Ruhlen 1994 and in other works. A. Dolgopolsky for one had employed the technique of certain lexical and/or grammatical types as Trombetti had used starting with his earliest published works.

The purpose of the present rather sketchy review is to acquaint readers of the *Mother Tongue* commemorative issue with global etymologies present in such publications as [Trombetti 1902, 1903; 1905, 1920, 1923, 1925] that have become rarities, despite such modern digitized versions as [Trombetti 1905] by the Google company.[1]

In his famous long-range studies, mostly in his native Italian, our scholar was wont to use such terms as *voci universale*, *tipi diffusi/diffusissimi* but not anything containing the term 'global'.

As regards taxonomy Trombetti operated with notions of 'languages of the Old World *vs* the New World, further with the realm of the Boreal (Northern part of the Globe) languages (Italian *ramo boreale*) and Southern (part of the Earth)' tongues (*ramo australe* in Italian). Throughout the works by Trombetti one also comes across the nine-fold partitioning of all the world's languages. Examples thereof follow: **IV** Indo-European languages, **V** Ural-Altaic tongues, **VI** Dravidian and Australian languages, **VIII** Munda-Polynesian tongues, **IX** languages of the Americas. This taxonomy of his demands a more detailed explanation and review, but for the purposes of the present article I mostly stick to the *taxa* — albeit hypothetical — current in the long-range research of our day, *viz.* Nostratic, Nilo-Saharan, (Congo-Saharan in some instances) Austric or Amerind super-stocks or *macrophyla*.

---

[*] Russian Academy of Sciences, Senior Research Fellow (Oufa local branch, Bashkortostan). nafiqoff@mail.ru

[1] Free e-book: http://books.google.com/books/about/L_unit%C3%A0_d_origine_del_linguaggio.html?id=pBRZAAAAMAAJ

The global etymologies researched by Alfredo Trombetti are truly voluminous. This being so, I have selected but a small sample, chiefly following the Swadesh standard 100 word list, with some asides occasionally made from his original 216 word list. This article consists of the factual data section, some parts *à la* discussion genre, followed by conclusions and the sources and references section. Now we commence with the **lexicon** fragment.

Global type NEG 'see/eye' [Trombetti 1920:325]

This particular lexical type was first mentioned in [Trombetti 1903:165, № 639], the earliest of the macro-comparative studies that I am cognizant of. The scholar calls this item *tipo diffusissimo* and in the sense of 'see' (v.) referring to *nak*, *nag* and the like as one of the more diffused types in [Trombetti 1905:193]. *Note*: henceforth the rendering from Italian is by the present author.

The Global etymology is to be inferred from:

I. Nostratic and Sino-Caucasian: Georgian and Laz *naχ*, Athapascan *naga, naχal* and similar glosses, Coptic *nau*; Berber *enh* – Tamil *nōkka* – Uralic *näge*; Nivkh *nju* 'see', *nax* 'eye'. [Note: in Trombetti's taxonomy Nivkh, with the other 'Paleo-Siberian' languages, forms a part of the 'American Indian tongues' macrophylum – *this author*]. Bodo *nai* 'watch, observe', *nu* 'eye', Chinese **m-in* 'face'.

II. Austric: Ainu *nak-aru*, *cf.* Melanesian *nago* 'viso'…

III. Australian [chiefly Paman-Nyungan, this writer's note] *nakk*; *nak*.

IV. Amerind[◊]**:** Algonkian *ene-*, *new* 'see', Carib *enu*, Costano *inu* 'eye', Wappo *nao* 'see', Yurok *neγwo* 'see'; … From here on the samples I have picked are far smaller than Trombetti's *comparanda*.

Type TI(G) 'see/eye' [Trombetti 1920: 267-269]

I. Nostratic: Svan *the*, Georgian dial. *tho-l* 'see'; Latin *tuē* 'guard' v.

Ia. Sino-Caucasian: Kuki *mi-t* 'eye'.

II. Austric: Dayak, Bugi, *i-ta*, *mi-ta*, Tagalog, Bisaya *ki-ta*; Khmer *prē-ta* 'see'; Thai *ta* 'see'.

IV. Amerind: Chinook *tai*, Seri *i-to* 'eye'; Aztecan *itta* < **ite-wa* 'see'.

V. Indo-Pacific: Andamanese: Bea *i-tā*, Bale *i-toa* 'see', Juwoi *re-tau* 'faccia'; Papuan: Gaima *tao* 'eye' v.

VI. Nilo-Saharan: *i-to* 'see', etc.

[◊] The Roman IV here stands for Trombetti's number IX, by and large the Roman numbers for the *taxa* are specific to this writer.

Global type GI, GU 'eye' [Trombetti 1920:163-166]

[Left out by this author for MT readers to see for themselves.]

Global type LAP, LA(M)B 'lick' [Trombetti 1920: 288, 289].

The lineage of this word may be glossed from the following extracts of the work being cited:

I. Nostratic: Dinka[2] *låp*, perfective *lap* 'lick', Somali *lēf* 'lick'; Indo-European: *laph-* : Armenian *laph-em* 'lecco', Albanian *l'ap* 'lick water', Russian dial. *lopa-tj* 'fressen'; Indo-European [again]: *lab-*: Old German *laff-an* 'lick', Saxon *lap-in-an* 'drink' v., Old Icelandic *lep-i-a* 'lick like a dog', Old Slavic *lobūzū* 'kiss' n., Latin *lambo* 'lick'.

III. Austric: Dayak *djelap*, Bisaya *dilap*, Bugi *lēpa* 'lick'.

VI. Congo-Saharan [after E. Gregersen]: Bantu *lamba* 'lick', 'lambire' : Swahili, Pokomo *lamba*, Sukuma *ramba*, Sotho *lapa* 'lick' … Zulu *lamba* signifies 'be hungry' , Duala *laba* means 'bite' v.

Global lexical type LE, LEME, LEBE 'tongue' [*ex* Trombetti 1920: 289]

By its appearance this type is similar to the one above, with occasional identity in semantics *quod vide infra.*

I. Nostratic, the Afroasiatic branch: Saho *an-rab*, Afar *ar-rabā*, Somali *ar-rab*, Galla [Oromo] *al-lābo*; Nandi *nge-liep*, Bari *ngé-dep*, Dinka *liéb, liép;*

Ia Sino-Caucasian [branch of the hypothetical Borean 'super-macro-family']: Udo [Udi] *lam-* 'lick', etc.

VI. Congo-Saharan: Kanuri *lam*, dial. *ta-lam*, *te-lam*, Maba *de-lmi-k*, *ta-lme-k*; cf. Fur *dā-li*, Wolof *lamei*, Mose *zi-lam-de*; Bantu: *-leme* 'tongue': Sotho *le-leme,* and so forth.

The root is also fairly discernable in the samples adduced by A. Trombetti earlier [Trombetti: 1903: 163]: Sino-Caucasian: Thuš [Batsbi] *lew-ar* 'speak'; Nostratic: Finnish *lan-sa*, Manchu *leo* – 'idem'; Austric: Vietnamese *loi*, *kai-loi* 'discourse'; Thai *lāu*; Karen *lau* 'speak'. In Trombetti's words *"la medesima radice anche nell' Oceania"* (loc. cit.).

The concept of 'tongue' as a somatic term may be expressed by a number of roots, as is well known. This is exemplified in many long-range studies by A. Trombetti, say in his most famous monograph which saw print in 1905 [Trombetti 1905, *passim*]:

I. Nostratic, Altaic branch: Turkish *dil* 'tongue'.

II. Austric: Austronesian family: *dila* 'tongue', Iloco *dil-dil* 'lick'.

III. Australian: Walookera *ū-tala* 'tongue', etc.

IV. Amerind: Wintu *talal*; Chon *tāre*, *k-tal*, *del* (dial.) 'tongue'.

V. Indo-Pacific: Bale *aka-atal*, some other Andamanese languages – *tal.*

VII. Khoisan languages: Bushman [//Au//en] *tari* 'tongue'.

---

[2] For Trombetti Dinka and other Nilotic languages were included (as "Hamitic") with Afroasiatic [Ed.].

Sino-Caucasian macrofamily: Garo *telai*, etc.

By the end of the twentieth century the reader comes across similar comparisons in works of several macro-comparativist scholars. Here is a sample published in [Blažek 1979: 34, 40]:

Proto-form *_tal_: ~ _dali_ 'tongue'
Sino-Caucasian macrofamily: Sino-Tibetan **dlag* 'tongue'.
Austric: Austronesian **dilah* 'tongue'.
The **Congo-Saharan** superstock's Nilo-Saharan branch **dali/mi/* 'tongue';
Kordofanian: Tumtum *djāro* 'idem' , etc.

The *comparanda* presented reflect a substantial refinement over Trombetti's parallels with the selfsame root, namely scholars of our day insist on comparing proto-forms wherever available, though certain attempts in this direction had been made by A. Trombetti himself, *quod videt supra*.

Worthy of note are frequent cases of direct continuity one observes in works of Trombetti and some later long-rangers: compare global roots KAP-, KOP- '*capere* ', KAP-, KAB- '*afferare col denti*' [Trombetti 1920: 125, 127] with Eurasiatic **kap* 'seize' [Greenberg 2002: 142, No. 331].

In the well-known work [Bengtson & Ruhlen 1994] we come across the global etymology of *KUNA* 'woman'. This widespread ancient root had been first explored by A. Trombetti. In his early research [Trombetti 1903: 155] he wrote about the universal spread of the type **γυνή** containing the word in question. In the famous book that produced a veritable sensation [Trombetti 1905: 179-100] the scholar present the following: according to his opinion the Ancient Greek word for 'woman' and the like present a composite name KU (*kui*, *kua,* see 'man') and *NA* (*nai*). In Mongolian [*sive* Altaic branch of Nostratic] *kű maű* 'man' is opposed to *kű-nej*, *kű-ni* 'woman'.

Indo-European: Sanskrit *gnā* 'wife of a god', cf. English *queen*.
Afroasiatic: Dembea *kwinā* 'woman', Chamir *dzenā* 'mother'.

Trombetti continued with more examples: *"Il medesimo tipo è rappresentate anche nell' Oceania"*: Nancowry *kān*, *kāne* 'woman'; Ulava *keni* 'idem'.

[**Indo-Pacific**]: Andaman *chana* 'woman', Bea *chāna–da* 'mother'.
Australia: NW coast *gīnaia*, Queensland in female names *in-gun*, as in Urgilla-*gun*. Tasmania *quanna* 'woman' [citation over].

Below is a sampling taken from the previously mentioned "Global Etymologies' [*loc. "cit.*]:

Proto-Afro-Asiatic **k(w)n* 'wife, woman': Kaffa *gen̄e* 'lady', Dembia *kiūnē* 'wife'; Oromo *qena* 'lady'; Akkadian *kinī–tu* 'wife' [one of a harem], Berber

(Tuareg) _tēkne_ 'wife'; Proto-Indo-European *_g^{w}en_ ~ *_g^{w}enā_ 'wife, woman'; Sanskrit _gnā_ 'goddess'; Avestan _gənā_ 'wife'; Slavic _žena_ 'wife, woman'; Lydian _kâna_ 'woman, wife'; Proto-Turkic *_küni_ 'wife' [one of a harem]: Kirghiz _künü_ 'wife'; Eskimo (Alaskan) _aganak_ 'woman'; Proto-Caucasian *_q(w)änV_ 'woman'; Andaman: Bea _chána_ 'woman'; Tasmanian (SE) _quani_ 'wife, woman'; Australian Aboriginal Warrgamay _gajin_ 'female'; Shawnee _kwan-iswa_ 'girl'; Dakota _hun_ 'mother'; Cayuse _kwun-asa_ 'girl'; Zuñi _k'anak^{w}ayina_ 'woman'; Tonkawa _k^{w}ān_ 'woman'; Zapotec _gunáa_ 'woman'; Proto-Tupi *_kuyã_ 'woman', Guarani _kuña_ 'female'.

Many more Amerind forms are in [Greenberg 2000, 47, also included there are Indo-Pacific and Australian glosses].

In [Bengtson & Ruhlen 1994; 291] the job of identifying numerous widespread roots is linked to and compared with pioneering works of Trombetti, Greenberg and other long-range scholars.

So much for the **lexical** Global etymologies proposed in various works by A. Trombetti.

Grammatical *comparanda*

The body of grammatical items in Trombetti's *Comparazioni lessicali* [Trombetti; 1920] is by far more modest *vis-à-vis* the lexicon therein. I do not wish here to treat at length such classical items of grammar as *pronomina interrogativa, personalia et cetera*. I am limiting myself to just a few instances. One of these is the global distribution of the interrogative I, U '*chi*?' ['who?': Trombetti 1920: 435].

Grammatical comparisons containing pronouns in this meaning – that is 'who?, what?' are to be found in many writings of the renowned scholar. The following is an extract taken from [Trombetti 1925: 87, 88]:

I. Nostratic, Altaic and Uralic branch: Turkic *ne* 'what?', 'which?', Tungusic *nī* 'idem'; Uralic Koibal *nō* 'what'; Afroasiatic: *kuna*; cf. demonstrative *_ʔi_ [Illich-Svitych 1971].

II. Austric, exemplified by Khasi *ka-no* 'quale', Vietnamese *nà–o* 'che?', Tagalog *s-no* 'chi?'.

Sino-Caucasian macrofamily: Basque dial. *no*, ergative *no-k* 'chi?' < *_na-n_ 'chi questo?'; Literary (Mandarin) Chinese *na* 'quale?'.

Niger-Congo: Bantu *-a-ni* 'chi?'

In the research of merited experts in long-range studies we also meet grammatical data similar to those given by Trombetti in his works. Suffice it to mention here interrogative etymologies No 10, 17 in [Bengtson, Ruhlen 1994] and No 60 'interrogative *K*' offered in [Greenberg 2000] where Eurasiatic *etyma* are coupled with Khoisan, Nilo-Saharan and other data.

Formally speaking grammar items of global scope are a common feature in Trombetti's legacy, say, in [Trombetti 1923, § 667 – 670] the reader learns about the global distribution of the expression of gender through vocalic variations, in § 678 global data on locatives are to be found.

In lieu of discussion

Alfredo Trombetti, father of global etymologies, was a comparative linguist whose works were well ahead of his time, little doubt about it. We ought to bear in mind the milieu reigning supreme in the late nineteenth and early twentieth centuries in comparative linguistics. I mean here the supremacy of the *Junggrammatiker* School which would look askance at any comparative research falling short of their meticulous *Lautgesetze* and virtual Eurocentrism. As great as Trombetti was, A. Meillet issued a highly critical appraisal of [Trombetti 1922-1923, 2 volumes] – a seminal monograph indeed. Italian colleagues of Trombetti, though not all, were no better. It was only near the end of his life that Trombetti had been conferred the title of Academician in the Italian Royal Academy of Sciences.

Nowadays the method of research Trombetti employed is widely known as 'mass comparison'. Yet, in an overwhelming number of instances, it has not been the notorious 'mess comparison' as sceptics would have it. By way of illustration I give only one more sample taken from [Trombetti 1905: 193], where glosses with the sense of 'fat' are represented in many of the world's tongues by various types: *pi* (Kunama, Indo-European, Thai, etc.); *kū* – (Bantu, Finno-Ugric); *sim-* (Hamito-Semitic [Afroasiatic] and Altaic). With this presented to the reader Trombetti comes with a very keen observation: *le concordanze sono fra lingue troppo remote fra di loro per potersi ammettere fin d'ora una connessione storica.*[3]

As pointed out on several occasions above many global etymologies by Trombetti have been borne out by eminent long-range researchers of later times. Thus, a global root *T* 'tooth' [Trombetti 1920: 218] was investigated in a masterly manner in [Dolgopolskiy 1964: 60]. An excerpt may be in place here: 'root' *T* is also in Yukaghir *todi* … || Altaic: old Ujghur *tiš* 'tooth' …

Trombetti is also said *inter alia* to have independently of E. Sapir suggested a superfamily now known as Dene-Caucasian, his insights on Indo-Pacific were also ahead of his time…

Certain faults and/or moot points were also present in the eminent scholar's researches. Say a case of contamination of two distinct roots can be seen in Gothic *qēns* [k^wēns] 'wife, woman', Old Icelandic *kona,* Old Irish *ben*, *vs.* Tamil *pen* 'female', Palaung *ī–pan*, Empeo *banāu* 'wife' [Trombetti 1905: 66, 67],[4] ad **kuni*, etc. 'woman'. Since R.

---

[3] ['the cognates are between groups which are too remote for a historic connection with each other to have been accepted so far', i.e. 'so far', but now, in Trombetti's view, they can be accepted as cognates. Thanks to J. Morris for translation. Ed.]

[4] As is well known to Indo-Europeanists, Old Irish *ben* is a regular development from PIE **g^wen-*; while the Tamil and following words seem to reflect a primeval labial [Ed.].

Caldwell's time *pen* has been held to be a distinct root in its own right by many fellow long-range scholars, notably by V. Illich-Svitych and others.

Concluding remarks

1. Alfredo Trombetti, a long-range comparative linguist *par excellence* of the past, was among the few first pioneer researchers who performed a many-faceted survey of the world's diverse languages *in toto*.
2. The famous Italian comparativist scholar, a Semitologist by his first trade was a staunch adherent of the theory of monogenesis, meaning a single primordial origin of humanity's tongues.
3. In order to bear this conception out Trombetti had come forward with numerous proofs both of lexical and grammatical nature; being the forerunner of the modern 'global etymologies' trend in the world science of language, more precisely in the field of the long-range ethnolinguistic comparison.
4. Many universally distributed roots (types) discovered by this eminent scholar have and are currently being corroborated and refined by a number of linguists from various countries, the present writer being one of their kin.

References

Bengtson, John D. & Merritt Ruhlen. 1994. Global Etymologies. In: M. Ruhlen *On the origin of languages: Studies in linguistic taxonomy*. Stanford: Stanford University Press.

Blažek, Václav. 1989. A survey of global etymologies. In: V. Shevoroshkin, ed. *Reconstructing languages and cultures*. Bochum: Brockmeyer.

Dolgopolsky, Aharon 1970. A long-range comparison of some languages of northern Eurasia. In: *Trudy VII Meždunarodnogo kongressa antropologičeskikh i etnografičeskikh nauk. Tom 5*. Moscow: Nauka, pp. 620 – 626. [This is a rare English version from the pre PC era.]

Fleming, Harold C. 2005. Some Comments in Trombetti by a Modern Long Ranger. *Mother Tongue* 10: 108.

Greenberg, Joseph H. 2002. *Indo-European and its closest Relatives. The Eurasiatic Language Family*. Vol. 2: Lexicon. Stanford: Stanford University Press.

Illich-Svitych, Vladislav 1971. *Opyt sravnenija nostraticheskix jazykov*. Vol.1. Moscow: Nauka.

Morris, Jonathan. 2005. Retrieving a Fore Father: Appreciating Alfredo Trombetti's Work. [A Trombetti Documentary.] *Mother Tongue* 10: 86-107.

Morris, Jonathan. 2006. Trombetti, Gatti, and the Birth of the Indo-Pacific Concept. *Mother Tongue* 11: 91-121.

Swadesh, Morris. 1960. On interhemispheric linguistic connections. In: *Culture in history: Essays in honor of P. Radin*, ed. S. Diamond, pp. 894-924. New York: Columbia University Press.

Trombetti, Alfredo. 1902/1903. Delle relazioni delle lingue caucasiche con le lingue camito-semitiche e con altri gruppi linguistici. In: [I] *Giornale della società asiatica italiana* XV (1902), [II] *Giornale della società asiatica italiana* XVI (1903).

Trombetti, Alfredo. 1905. *Unita d'origine de linguaggio*. Bologna: Treves.

Trombetti, Alfredo. 1920. Comparazioni Lessicali. In: *Saggi di glottologia generale comparata*. T.3. Memorie della R. Accademia delle Scienzi dell' Istituto di Bologna. Sezione di Scienze Storico Filologiche, Ser.2.

Trombetti, Alfredo. 1923. *Elementi di glottologia*. Vol.2. Bologna. Zanicelli.

Trombetti, Alfredo. 1925. *Le origine della lingua basca*. Firenze.

MOTHER TONGUE
Journal of the Association for the Study of Language in Prehistory • **Issue XVIII • 2013**
50th Anniversary of **J.H. Greenberg's *The Languages of Africa*** (1963)

Notes on the Moscow Conference on Long-Range Comparison[1]

Peter A. Michalove†
University of Illinois, Urbana

It was a great pleasure to attend the recent conference on "Problems in the Study of Long-Range Linguistic Comparison at the Turn of the Third Millennium" at the Russian State University for the Humanities in Moscow, May 29 through June 2, 2000. Led by Sergei Starostin, the conference was extremely well organized, with much of the logistical preparation made very effectively by George (Gosha) Starostin.

For me, one of the most fascinating aspects of the trip was the opportunity to see Moscow again for the first time since 1988. I had been there several times in Soviet days, and I was constantly struck by how much had changed since then, and by what had not changed. But that's a separate story.

The conference itself covered a number of topics. The first day involved papers on Indo-European. I felt that it was significant to devote an entire day to this best-established of language families at a conference on long-range comparison; the presentations made clear that work on established families is in principle no different from work on long-range work. Both endeavors share the same principles, goals, and problems.

The second day was devoted to Nostratic, and included papers on lexical, morphological, and phonological comparisons, as well as more theoretical considerations. After that the agenda became somewhat muddled as the schedule became more flexible to accommodate speakers who came late or left early, or were unable to come at all.

There was a very interesting session on Altaic, and Sergei gave an introduction to the Altaic etymological dictionary he is currently preparing in collaboration with Anya Dybo and Oleg Mudrak. The current state of the dictionary is available on the web at http://starling.rinet.ru/intrtext.htm, along with other etymological databases in progress.

Another new etymological dictionary presented at the conference was the Semitic dictionary being prepared by Yuri Militarev and L.E. Kogan. Afroasiatic linguistics was also discussed in several papers at a session on comparative linguistics and ancient near eastern history, held in memory of the late Igor Diakonov. There was also a session on Sino-Tibetan and Caucasian linguistics, which I missed because it was held at the same time as the ancient near east session. In all, the conference covered a wide range of topics, and the organizers will publish a book of the conference proceedings around the end of this year.

---

[1] Reprinted from *Long Ranger* (Newsletter of the Association for the Study of Language in Prehistory: formerly Mother Tongue Newsletter), Issue 33, Part 1, September 2001. Online: http://www.people.fas.harvard.edu/~witzel/LR33.htm

But of course the high point of the conference was the opportunity to see old friends and meet new ones. I was especially glad to meet Fabrice Cavoto in person; we have been corresponding by email for some time now. In addition it was good to see Sergei Starostin and Aharon Dolgopolsky, whom I had met before.

Martine Robbeets, who was studying in Moscow for a month, had the task of orienting the foreign visitors, a job she fulfilled admirably. I especially enjoyed talking with her, and with Egidio Marsico.

Among those whom I had known only from their published works, it was a pleasure to finally meet Václav Blažek, Vladimir Dybo (who was just elected to the Russian Academy of Sciences) and his daughter Anya, Thomas Gamkrelidze (whose Georgian charm and wit were very much in evidence), Eugene Helimsky, Alexander Lubotsky, Edkhiam Tenishev, and several others.

One of the students at the conference gave some of the foreign visitors a tour of Moscow for an afternoon. Good linguists that we were, we spent as much time excavating the local bookshops as we did seeing the sights of Moscow. We all came home loaded down with more books, and amazed at the contradictions that fill the streets of modern Moscow.

Book Review

Desi Words Speak of the Past: Indo-Aryans in the Ancient Near East, by Dr. Liny Srinivasan. Bloomington, Ind.: Author House, 2011. xxxv + 534 pp.
ISBN 146709479X, 9781467094795

The author has degrees from the University of Calcutta and University of Poona, India. She has held the position of Lecturer in Geography at Nistarini College, Purulia, India. In the United States she has further received degrees of M.A. from SUNY, Binghamton, N.Y., and Ph.D. from the University of Pittsburgh (both in Geography). She has also been a Fulbright scholar researching Hindu temples in Calcutta.

The book being reviewed here consists of fourteen chapters:

> I. Significance of Dešì Words. II. Mythological Geography of the Puranas. III. Near Eastern Names of Land and People in Ancient Indian Literature. IV. Historicity of the Indian Mythology; the Lost History of Canaan. V. The Lost Contexts of the Rigveda: the Biblibal [sic] World. VI. The Ilavrita Varsha; Biblical Pishon, the Lost River of Paradise and the Gold. VII. Was Sarasvatì Really the Nile? VIII. The Mythical Lands of Švetadvìpa and Švetavarsha; Testimony of the Indo-Aryans and Indo-Europeans. IX. The Land and People Kuru; Egyptian Khuru before Akhenaten. X. The Egyptian Contexts of the Epic Ramayana. XI. Mycenaean or Homeric Greek Names of Land and People in the Rigveda and in Indian Myths. XII. The Hurrian Empire Mittani [sic] and Kamadeva, the Mythical God. XIII. The Egyptian Connection of the God Kâma and the Spread of his Cult. XIV. The Significance of Mittanian [sic] Royal Names: a Peep into Their History.

At the outset the initial word of the title, *Desi*, may need clarification. Otherwise written "Dešì" in the book, and conventionally as *deśī* or *dēśī,* it comes from Sanskrit *dēśá* 'point, region, part; province, country' (CDIAL 6547; WP I 776) and in this context it means 'local'. "*All* that the [ancient Indian] grammarians could not figure out even with their excellent knowledge of [Sanskrit] and [Prakrit], was called *deśī* 'local'. As expected, these words include many substrate and adstrate words from the various non-IA languages of the subcontinent, but some of them may also be new formations …" (Witzel 1999, p. 121).

The author states that the "primary focus of this book is to restore the lost historic and geographic contexts of the accounts and myths of the ancient Indian literature. Additionally, another goal is to draw the attention of the scholars to the ancient Indian literature, particularly of the scholars of Near Eastern Studies, of Egyptology, of the Bible and of the scholars of ancient languages written in cuneiform scripts" (p. xiii). The author admits to a "superficial knowledge about Near Eastern Studies" (p. xiii), but nevertheless she posits a large number of far-reaching claims about historical and geographic connections between the Near East and the Indian subcontinent.

The author's fascination with these topics apparently began with "finding out the existence of a massive number of Canaanite and Egyptian words in Bengali, I realized the need to uncover the historical connections. … Everything I learned was against my conventional notions. Confused and terrified, I ran to Professor Gordon" (p. xi).

Cyrus Herzl Gordon (1908-2001) was a recognized authority on the eastern Mediterranean, Semitic languages (especially Ugaritic) and Egyptology. He was also interested in "non-traditional viewpoints," or what some would call "fringe theories."[1] Gordon took an interest in Liny Srinivasan's discoveries, and they collaborated on an article about the purported "Canaanite vocabulary" in Bengali and in other Indo-Aryan languages, which was published in the first (1995) issue of this journal. This article was critically examined by Witzel (1999b) several years later.

The author's methodology is summarized by a typical quote:

> Similarity of some ethnic names of the Puranas with Near Eastern names of ancient people has already been shown in many previous articles, but that is not enough for some scholars who argue for coincidental occurrences and who are against comparison of names like that of Sarádanas (Markandeya) and Sarâdhânas (Brihat Samhita) with Egyptian Srdn for Cuneiform Sardanu, Kuhakas (most Puranas) with Egyptian Khk 'Kehek', and Kirátas (Mahabharata) with Biblical Keretheim. They have only the reason that the names are not phonetically cognate. … It is like Calcutta is pronounced as Kolkâtâ by Bengalis, but it is still the same city when the Hindi speakers pronounce it as Kalkattâ or some Dravidians pronounce it as Kâlkuttâ. The sense of the words and the number of consonants justify their comparison. Also, from the point of view of statistics, the massive number of such similar names as shown throughout this book can not be accidental (pp. 58-59).

It is not clear what "phonetically cognate" means. (The author, p. 2, also states that "vowels are not important.") To an historical linguist words are 'cognate' if they share a common origin, and in many cases cognates in this sense may have lost phonetic similarity. Who would ever guess that English *wolf,* Albanian *ujk,* and Persian *garg,* with no consonants in common, were cognate, but they are indeed (< PIE _*u̯lkʷos_ ~ _*lukʷos_ 'wolf': Buck 3.71; WP I 316). Does "the number of consonants" make a comparison "phonetically cognate"? To take a hypothetical example, there is an English family and place name *Featherstonhaugh* that is pronounced /fænšɔ:/. If not for centuries of written tradition it would be spelt _*Fanshaw_, and by purely look-alike criteria ("the number of consonants") it would seem reasonable to compare it with, say, Scandinavian *Finnskōg* ('Woods of the Finns', in parts of Norway and Sweden) and conjecture that Finns had at some time settled in English forests. Unfortunately, this kind of "method" is essentially the one usually followed in this book.

For an example of the author's "phonological method" I shall quote a complete paragraph:

> Euphrates has two types of names. Series one starts with /p/ and ends in /t/ or /th/, such as Perath in Hebrew, Puranti in Hittite and Hurrian, and Puruttu in Akkadian. In spite of all these variations they reflect one original source, which becomes evident from the careful scrutiny of the Hebrew spelling Perâṯ. The end letter is Tasda [sic] /ts/ which in the initial is generally vocalized as a sibilant /s/ but in the middle or end as /th/. In the context of the Rigveda, it stands for "o "an unique spirant [sic], but rendered as /sh/ in English. Dešì Bengali vocalizes it as a cerebral š with an additional nasal, that is š + ñ, such as Hebrew ṯor 'bull' and Bengali shãr 'bull' (where ã = nasalized a+ ñ). More examples can be found in the Bengali book (Srinivasan, 1903). Parushñi written with this /s/ and combined with a special /ñ/ could easily be simplified to Pura-nti by

---

[1] http://en.wikipedia.org/wiki/Cyrus_H._Gordon

> metathesis of /sñi/ of the Rigveda as /nti/, since there is no way to write the Rigveda name in cuneiform. The Akkadian doubling of /t/ was another way to indicate a different /t/ that is /th/ of Hebrew. Unfortunately, the older Canaanite form of vocalization is unknown. There is another series of names for the same river, such as, Uruttu in Subartu (Gelb, p.21), Ufratu in Old Persian, Purat in Mandaean, Furat in Arabic, Firat in Turkey and Euphrates in Greek. The Arabic and Turkish names similar to Hebrew Perath could come after it, but the older variants of Ufrata and Euphrates could come from a different source as follows (p. 172).

I have read many linguistic treatises over some five decades, but I cannot recall any passage as opaque as this. So far so good, until one arrives at Hebrew "Perâṯ. The end letter is Tasda /ts/ …" In fact, the last letter of Hebrew פרת *P*^{e}*rāṯ* is not Tsadi (צ) but Taw (ת), which indeed traditionally has a "th"-sound [θ] in non-initial positions (realized as /s/ in Ashkenazi tradition, /t/ in Sephardic and Israeli Hebrew). But the next part ("In the context of the Rigveda, it stands for "o "an unique spirant, but rendered as /sh/ in English") is completely mysterious to me. (Perhaps the "o" is a misprint for the retroflex /ṣ/, which would make sense in this context.) Then we come to "Deši Bengali vocalizes it as a cerebral š with an additional nasal, that is š + ñ, such as Hebrew ṯor 'bull' and Bengali shãr 'bull'." First, the Hebrew word for 'bull' is not "ṯor" but שור *šōr* (= Aramaic תורא *tōrā* 'ox, bull', etc. < Proto-Semitic **ṯawr-* [Militarev] or **'θawar-* [Dolgopolsky]).[2]

The comparison of the Semitic words for 'bull' with Bengali (Deśī) *sā̃ṛ* 'bull' (Srinivasan & Gordon 1995, p. 202) was criticized by Witzel (1999b, p. 46), pointing out that the *deśī* word *sā̃ṛ* (with a long nasal vowel) comes from Middle Indo-Aryan (Aśoka) *saṁḍa(ka)* 'bull set at liberty', attested earlier in *vṛddhi* form as Vedic *sā́ṇḍa* 'uncastrated (of bull)' (CDIAL 13331). Some modern Indo-Aryan languages, such as Oriya *saṇḍa*, still exhibit the more archaic form with an internal retroflex nasal-stop cluster. So applying one of the cardinal rules of etymology, that *cognate words should become more similar the further they are traced back*, it becomes abundantly clear that Semitic **'θawar-* and Old Indic *sā́ṇḍa* cannot be cognates, whether by genetic transmission or borrowing.

Since the purpose of the quoted paragraph was apparently to establish some kind of "regular phonetic correspondence" between Semitic **t* (ת) or **ṯ* = **θ* (which is not clear) and Indic **šñ,* better [*ṣṇ*], the author's end purpose, to identify *P*^{e}*rāṯ*, etc. 'Euphrates' with Indic *Paruṣṇi* (confusingly written "Parushñi"), one of the five great rivers of Punjab now better known as *Ravi*, does not work either.

Examples of the author's method abound throughout this book. In this brief review I can only discuss a few of them.

- Coptic *tal* 'mound, small hill', Akkadian *tulu*, Hebrew *tel* id. are compared with Old Bengali *tâl* 'hillock, small hill', *tâlat* 'on the hillock', Modern Bengali *tilâ* 'hillock, small hill' (p. 3). The Coptic and Semitic words belong to a well-known Afroasiatic etymology: Proto-Afroasiatic (PAA) **tVl-* 'hill' > Cushitic **tul-* id., West Chadic (Hausa) *túllúwā́* 'top (of the hill)', Proto-Semitic **tall-/*till-* 'hill' > Akkadian *tīlu*, Ugaritic *tl*, Hebrew *tēl*, Syrian Aramaic *tell-*, Arabic *tall-* id.; Coptic Sahidic *tal, tol* 'hill' < New Egyptian *tnr* 'Ort wo Kraut wächst', probably <

---

[2] Obviously similar to Latin *taurus*, Greek ταῦρος, etc., which Dolgopolsky and other Nostraticists consider a loanword from Semitic, while English *steer*, Avestan *staora* 'large cattle', etc. are genetic cognates (*Nostratic Dictionary*, no. 409). The loan of the Aramaic word to Latin and Greek is also mentioned by Srinivasan & Gordon (1995, p. 202) in connection with the westward spread of bullfighting.

Semitic (V 213).[3] Though not indicated by the author, the Bengali words have a retroflex initial /ṭ/:[4] cf. Sindhi *ṭalo* 'raft of timbers', Nepali *ṭāl* 'pile of wood', Hindi *ṭāl* 'pile of wood'; Lahnda *ṭillā* 'hillock', Panjabi *ṭillā* 'hillock, mound, heap of grass or timber (used as a raft), raft', Hindi *ṭīlā* 'hillock, mound', etc., all < Old Indic (hypothetical) **ṭalla / *ṭilla* (CDIAL 5451; Bengali words are not cited). The initial /ṭ/ of the Indic words indicated that they are not of IE origin, thus genuinely *Deśī*. There is really nothing wrong with the comparison, on the face of it, but it seems unlikely to this reviewer that Afroasiatic dental /t/ would be borrowed as Indic retroflex /ṭ/, or vice-versa.

- Coptic *ney* 'see, look' is compared with Prakrit *nia, niao* 'see, seeing' (p. 4, table 2). Coptic has several dialects, among which the relevant forms are Sahidic, Bohairic *naw*, Fayumic, Lycopolitan *new*, Akhmimic, Lyc *no* 'to see' < Ancient Egyptian *nw* < PAA **naʔ-/*naw-/*nay-* 'to see' (V 144, 147; Morris 2011: 188). It is clear that the Coptic words represent an ancient Afroasiatic root. On the other hand, the cited "Prakrit *nia, niao*" seems to stands for Prakrit *ṇi(b)hālēi* 'sees' (cf. Nepali *niyālnu* 'to look', etc.), and comes from an Old Indic form such as *nibhālayati* 'perceives', in which the *ni-* is a prefix and the root is *bhal-* 'to look' (CDIAL 7228). So when origins of both words (Coptic and Prakrit) are analyzed, it becomes apparent that they have no connection.
- Coptic *shant* 'nose' is compared with Prakrit *sundhio* 'smelled' (p. 4, table 2). The relevant Coptic forms are Akhmimic *šeent, šent*, Bohairic *šai*, Sahidic *šaant-, šant-, šaat-, šat-, šnt-, ša* < Eg. (Pyramid texts) *šr.t* 'nose' < **širyat* (V 253-254). The "Prakrit *sundhio*" looks like Prakrit *suaṁdha* 'fragrant, fragrance', a development of Skt *sugandha*, i.e. *su-gandha* 'fragrant, perfume', etc. (cf. Hindi *saũdh* 'fragrance', etc.). The Indic word is composed of two elements: *su-* 'good' + *gandha* 'smell' (CDIAL 4014, 13454), while the Coptic word is a unitary morpheme and clearly not related to the Indic. Vycichl explains the nasal variants < **šint < šitt < širt < širi.t*, so the *-n-* in these words is secondary.
- Coptic *oynam* < Eg. *ymn* 'right side or hand' is compared with Prakrit *jemnoi* 'right side or limb' (p. 4, table 2). Eg. *i͗mn* (Pyramid) 'right (-hand), right side; West' is from a well-known AA root **yamin-* 'right hand' > Hebrew *yāmīn* 'right side', Arabic *yamīn-, yaman-* 'main droite; côté droit', etc. The Prakrit word looks like Gujarati *jamṇũ* 'right (not left)' < *jamṇɔ hāth* 'eating hand, right hand', related to Skt *jamana* 'eating' < *jam-* 'to eat' (CDIAL 5126). So the Indic words are derived from 'eating hand = right hand' and only superficially, and secondarily, similar to the Afroasiatic.

In a similar vein the author asserts: "Ancient [Indian] names are often prefixed with elements borrowed from Near-Eastern languages. The two most common prefixes /Vi/ and /Pra/ from the Rigveda onward seemed to have come from Egyptian *Pi* and *Pr*, and they both mean 'house'" (p. 261). Thus "Pragâtha is also a Rigveda rishi and composers [sic] of three hymns in the 8th book of the Rigveda. … The name indicates that once Pragâtha meant 'House of Gâtha'," which is identified with Gath (home of the Philistine giant Goliath) in the Old Testament (p. 262). Similarly *Vidarbha*, part of the Indian state Maharashtra, is interpreted as 'house of Darbha' (p. 264). Any student of Sanskrit knows that *ví* and *prá* are common verbal prefixes meaning 'apart, asunder, away, out' and 'forward, onward, forth, fore', respectively, and they are among the three most common prefixes in the "older (Vedic) language" (Whitney 1964, p. 396). *Ví* actually comes from PIE **wi-/*wī-* 'separate, alone', related to German *wider* 'against' (Skt *vitarám* 'farther off'), English *with*, etc. (WP I 312); and *prá* is also a well-known IE element, as in Latin *pro-*, Russian про- /pro/, English *for-, fore-, forth*, German *ver-*, and so on (WP II 29). *Pr* (Coptic *pōr*) is indeed an old Egyptian word for house (I have not been able to verify the form *pi*) but it has nothing to do with PIE **pro-*, etc. Old Indic *gāthá-*, *gā́thā* 'song', Pali *gāthā* 'verse, stanza' are simply derivatives of the root *gā(y)-* 'to sing' (WP I 526; CDIAL 4126), and have nothing to do with the Philistine city Gath.

---

[3] Unless indicated otherwise, Afroasiatic data come from Militarev (2005, 2006). I am also grateful for some corrections from V. Blažek.

[4] Rather strangely, for an author who is a native of India, the distinction between dental/alveolar and retroflex consonants is ignored throughout the book.

On the basis of similar sounds "Mount Mujâ-vat was very likely the same Mountain of Moses. … Arabic name Jebel Musa … the Rigveda Mujâvat was the name of a sacred peak, and the name of the entire Sinai plateau" (p. 60). In fact Mount *Mūjavant* or *Mauja-vant* 'having *Mūja* (people)' (Avestan *Muža*) is well known as the mountain from which the best *Soma* (= Av. *Haoma*, the ancient Indo-Iranian ritual hallucinogenic herb) came. The Tibetan form *bru-źa* and Sankritized form *puruṣa* allow a possible identification with Burushaski *Burúśin* 'Burushaski speaker', plural *Burúśo*, making *Mūjavant* '(Mountain) having Burushos' (Witzel 1999b: 5); alternatively *Mūjavant* may be identified with mountains of similar names in the ranges north of India (Witzel 2012, p. 159, note 440). In any case there is no evidence to support a source of *Soma/Haoma* in Sinai, and there is no mention of the ritual herb in Srinivasan's Chapter III.

The author admits to a "superficial knowledge about Near Eastern Studies" (p. xiii). Nevertheless, when attempting a work of this scope some effort should be made to consult existing etymologies in the languages concerned. As noted already by Witzel (1999a: 138) the author apparently still does not use the available Indo-Aryan and Indo-European etymological dictionaries (e.g. CDIAL; WP; Mayrhofer 1956-76), which are not listed in the references.

As mentioned above the author's "goal is to draw the attention of the scholars [in Near Eastern studies] to the ancient Indian literature," but unfortunately the methods employed in this book make it very difficult to be used effectively. It is not enough to juxtapose similar sounding words and names. One must also show that the lexemes are plausibly connected by employing etymological and morphological analysis. In the case of myths and stories there has to be enough structural and historical analysis to demonstrate that the narratives being compared are credibly connected in origin. This reviewer is an outsider to both fields concerned (Near Eastern studies and Indic studies), and it is possible that experts in these fields might be able to extract some useful information from Dr. Srinivasan's book, but I am not very hopeful of this.

References

Buck = Buck, Carl Darling. 1949. *A Dictionary of Selected Synonyms in the Principal Indo-European Languages*. Chicago: University of Chicago Press.

CDIAL = Turner (1962-66)

Dolgopolsky, Aharon. 2008. *Nostratic Dictionary*. University of Cambridge, Department of Archaeology. https://www.repository.cam.ac.uk/handle/1810/196512

Mayrhofer, Manfred. 1956-76. *Kurzgefasstes etymologisches Wörterbuch des Altindischen.* Heidelberg: Carl Winter Universitätsverlag.

Militarev, Alexander. 2005. *Egyptian Etymology Database*. Tower of Babel. http://starling.rinet.ru/cgi-bin/main.cgi?root=config

Militarev, Alexander. 2006. *Semitic Etymology Database*. Tower of Babel. http://starling.rinet.ru/cgi-bin/main.cgi?root=config

Morris, Jonathan Sherman. 2011. "The myth of rapid linguistic change IV: The evidence from Egyptian." *Mother Tongue* 16: 137-197.

Pokorny, Julius. 1959-69. *Indogermanisches etymologisches Wörterbuch*. 2 vols. Bern: Francke Verlag.

Srinivasan, Liny Asoka. 1999. "An Enquiry about Sumerian Words." *Mother Tongue* 5: 109-118.

Srinivasan, Liny & Cyrus Gordon. 1995. "Canaanite Vocabulary in Bengali and in Some Other IE Dialects of India." *Mother Tongue* 1: 202-206.

Turner, R. L. 1962-66. *A comparative dictionary of Indo-Aryan languages*. London: Oxford University Press.

V = Vycichl, Werner. 1983. *Dictionnare étymologique de la langue copte.* Louvain: Peeters.

Whitney, William Dwight. 1964. *Sanskrit Grammar*. (10th issue of 2nd Edition [1889].) Cambridge, Mass.: Harvard University Press.

Witzel, Michael. 1999a. "Sumerian and Bengali? Some Methodological Problems in Long Range Comparisons." *Mother Tongue* 5: 119-140.

Witzel, Michael. 1999b. "Early Sources for South Asian Substrate Languages." *Mother Tongue* Special Issue: October 1999: 1-71. [Pp. 46-47 remarking on Srinivasan & Gordon 1995]

Witzel, E.J. Michael. 2012. *The Origins of the World's Mythologies.* New York: Oxford University Press.

WP = Pokorny (1959-69)

Association for the Study of Language in Prehistory (ASLIP)

News and Notices

ASLIP Annual Meeting 2013

The ASLIP annual meeting was held the 9th of November, 2013, at the Harvard University Sanskrit Department, 1 Bow Street, Cambridge, Mass. Attending were Michael Witzel (President), John D. Bengtson (Vice-President), Michael T. Lewis (Secretary-Treasurer), Harold C. Fleming (Board of Directors), B.K. Rana, and Nicholas Davidson.

The non-profit corporate status of ASLIP, temporarily suspended as reported at the last meeting, is still in process of being resolved, with President Witzel and Secretary Lewis working to restore non-profit status.

The improvement of ASLIP's electronic presence – updating and modernizing the ASLIP homepage (http://aslip.org) and electronic publication of *Mother Tongue* – are also in process. Brita Bengtson, Technical Advisor, will continue to be involved in further electronic development of ASLIP, and has already updated some parts of the homepage.

There was a discussion of what the policy of *Mother Tongue* should be regarding peer review of articles. At one extreme would be acceptance of virtually everything that is submitted, in the name of not suppressing free discussion; at the other pole would be a peer review process so tightly controlled that any serious discussion of topics like monogenesis (or polygenesis) of language, global etymologies, and macrofamily hypotheses would be excluded by definition. The difficulty for *Mother Tongue* is that there is a very small pool of reviewers capable of handling our special topics. In an email message received shortly before the meeting a member stated the opinion that the "self-peer-reviewing practice [between and among frequent *Mother Tongue* contributors] undoubtedly is much stricter than most reviewers elsewhere." This issue is still evolving as the 2013 issue (MT XVIII) is in production.

Another discussion revolved around the general mission of ASLIP. Nicholas Davidson asserted that "the *Zeitgeist* is developing in ASLIP's favor. Beginning points, such as the Big Bang theory and Mitochondrial Eve, are just as necessary for historical (genetic) linguistics as they are for other sciences. Upcoming generations of scholars will not accept the polygenesis of language and the purported existence of hundreds of 'unrelated' language families in the world. ASLIP and *Mother Tongue* are essential to the support and cultivation of an evolutionary approach to historical linguistics and its integration with other related sciences" [paraphrased by Ed.].

Finally, the yearly election of ASLIP officers and Board members was conducted. (See inside front cover of this issue for the slightly revised list.) A new office was also created, that of Information Officer, in charge of public relations and media connections. Jonathan S. Morris (São Paulo, Brazil) was nominated and elected as Information Officer. The meeting was adjourned and the attendees reconvened at the nearby Hong Kong Restaurant for lunch.

ASLIP's Electronic Front

As mentioned in the meeting minutes, this aspect of ASLIP's work is in transition and process. The ASLIP website (http://aslip.org), which has had outdated information (about officers, links, etc.) for years is gradually being updated and corrected.

It is important to note that Allan R. Bomhard, former ASLIP vice-president and editor of *Mother Tongue* (newsletters and journal) has done us the valuable service of scanning all the issues of *Mother Tongue (Newsletter),* from No. 1 (Nov. 1986) to No. 31 (Fall 1998). See: http://www.scribd.com/collections/4354715/Mother-Tongue-Newsletters

The name *Mother Tongue* was first formalized with the third issue. After issue 31 the newsletter name was changed to *Long Ranger*, continuing the same number order as *Mother Tongue*. Of these *Long Ranger* No. 32 was a print issue which is viewable on **scribd**. As far as we know this was the last print issue. Thereafter issues 33 (Part 1) and 34 (a, b) were electronic, and can be viewed on http://aslip.org.

Some issues of *Mother Tongue (Newsletter),* or parts thereof, can also be viewed on http://aslip.org, some in scanned form and some as html text.

Somewhat confusingly, in 1995 *Mother Tongue (Journal)* was initiated, so it ran concurrently (issues I–VIII) with the newsletter (issues 24–34) from 1995 through 2003.

The long range plan is to make back issues of *Mother Tongue (Journal)* available on the web, as well as to make current issues of *Mother Tongue* available by electronic subscription.

MT Press

At the 2012 ASLIP meeting Hal Fleming put forward a suggestion that ASLIP establish a publishing branch, tentatively called "MT Press." This would allow writers to bypass conventional publication and offer their work at a low cost – in tens of dollars rather than hundreds.

Authors interested in this option may contact *Mother Tongue* Editor John D. Bengtson for more information: palaeojdb@hotmail.com

Back Print Issues of MOTHER TONGUE

Back print issues of *Mother Tongue* are available for purchase. The following table summarizes some of the topics covered in issues I – XVII:

I (1995) **Inaugural Issue:** Canaanite & Bengali, Austric; Basque & Dene-Caucasian (R.L. Trask & 12 discussants); Proof in Genetic Linguistics (Greenberg)
II (1996): Kusunda, Ainu, Basque, Nihali (Mundlay & 8 discussants); Basque & Dene-Caucasian (S. Starostin, Trask, Ruhlen); Multilateral comparison (Greenberg)
III (1997): Kusunda, Nihali, Sumerian; "Hardware" / Origin of Language Symposium

(Zegura, Lieberman, Donald, Fitch, Deacon); Recommendations for Long Rangers (Benedict); S.A. Starostin
IV (1998): Yeniseian; Ainu (Sidwell, Itabashi, Norquest, Bengtson); Deep classifications; Apophony (ablaut)
V (1999): Austric (Hayes, Blažek, Blust, van Driem, Fleming); Basque & Caucasian (Bengtson & 6 discussants); Sumerian (Srinivasan, Witzel, Diakonoff, Bengtson); Climatic influences on language; Bipeds, tools & speech; American prehistory
SPECIAL ISSUE (1999): **South Asian substrate languages** (Witzel, Whitehouse, van Driem, G.D.S. Anderson, Kuiper, Masica, Mundlay); Austronesian taxonomy
VI (2000/2001) **Festschrift for Roger W. Wescott**: Austric; Paleolinguistics: The State of the Art and Science (10 discussants); Obituaries: Wescott, Gordon, Greenberg
VII (2002) **In Honor of Joseph H. Greenberg**: Elamite, Dravidian, Ongota, Shabo, Tasmanian, Andamanese, Eurasiatic; Greenberg's taxonomic proposals; Proto-Human or Proto-Sapiens
VIII (2003) Linguistic Databases & Taxonomy Workshop (SFI): Nostratic, Salishan & Caucasian, Basque, Khoisan, Negative Evidence (Whitehouse); EHL Project
IX (2004): Australian languages, Kadu, Ongota, Shabo; Australian languages (O'Grady & Whitehouse); Proto-Sapiens kinship words: (P)APA, (T)ATA; Mario Alinei
X (2005): Kusunda, Basque, Eurasiatic; Obituaries: Livingstone, S. Starostin, Greenberg; Flores "hobbits"; Great Archeological Debate; Pre-Clovis site; Chinese genome; Trombetti
XI (2006) **Asian Remnant Languages & Year of the Australoid** (Harvard / ASLIP Conference, 2006): Indo-Pacific, South Asian languages, Tibeto-Burman, Austroasiatic, Kusunda, Austric, Australian, Dravidian, Andamanese; Archeology of Southern Route (Harrod); Out of East Africa by 77K BP (Brooks); Population genetics
XII (2007) **In Honor of Harold C. Fleming's 80th Birthday**: Indo-European, Nostratic, Kartvelian, Bangi Me, Shompen, Dravidian; Nostratic Phonology (Bomhard, Sidwell, G. Starostin); Obituaries (Orel, Helimski, Bender); Glottochronology, Genetics
XIII (2008) **Commemoration of Ann Arbor Language & Prehistory Symposium (1988):** Milyan, Nostratic, Uralic, Chukcho-Kamchatkan, Shompen, Andamanese; Obituaries: Zvelebil, O'Grady; Bio-genetics; Fallacy of time limit; Myth of rapid linguistic change; Linguistic chronology
XIV (2009) **Commemoration of Daniel F. McCall**: Indo-European, Caucasian, Basque, El Molo, Mesmes, mystery languages of East Africa; Berber *H (Fournet, Blažek, Kossmann, Prasse); Paleoanthropology; Myth of rapid linguistic change II; Numerals (Hurrian, Nilotic); Profiles (Dolgopolsky, Mallory)
XV (2010) **Fifteenth Anniversary Issue 1995-2010:** Areal patterns of myth motifs (Berezkin); Holocene etymology of 'pitch'; Myth of rapid linguistic change III; Yeniseian numerals; Afrasian etymologies; Review of Campbell & Poser *Language Classification*
XVI (2011): Archeology & Genetics; Indo-European & Fenno-Ugric (Pedersen); Chinese giant Pangu; Minoan; Milyan; Surmic numerals; Dene-Caucasian; Myth of rapid linguistic change IV; Review of Jones & Milicic *Kinship, Language & Prehistory*
XVII (2012) **In Memory of Aharon Dolgopolsky:** Personal memories of Aharon Dolgopolsky; Archeology & Genetics; Trombetti's "Puluga" and discussion; Kamchukchean and Eskaleutian; Discussion: The Number 'One' (Levitt, Blažek, Bomhard, Bürgisser, Janhunen)

Back print issues can be had for $10 (domestic U.S.) or $20 (foreign). Please contact ASLIP Secretary-Treasurer **Michael T. Lewis**, 20 Duane Avenue, West Newton, MA 02465, U.S.A. | Tel. 617-964-0978 | lewismtc@rcn.com

Book Notices

New Perspectives on the Origins of Language. Edited by Claire Lefebvre, Bernard Comrie and Henri Cohen.

Studies in Language Companion Series, 144. 2013. xvi, 582 pp. Université du Quebec à Montréal / Max Planck Institute for Evolutionary Anthropology.

> The question of how language emerged is one of the most fascinating and difficult problems in science. In recent years, a strong resurgence of interest in the emergence of language from an evolutionary perspective has been helped by the convergence of approaches, methods, and ideas from several disciplines. The selection of contributions in this volume highlight scenarios of language origin and the prerequisites for a faculty of language based on biological, historical, social, cultural, and paleontological forays into the conditions that brought forth and favored language emergence, augmented by insights from sister disciplines. The chapters all reflect new speculation, discoveries and more refined research methods leading to a more focused understanding of the range of possibilities and how we might choose among them. There is much that we do not yet know, but the outlines of the path ahead are ever clearer.

Table of Contents:

Part 1. General perspectives and issues on language origins

Historical, Darwinian, and current perspectives on the origin(s) of language
Henri Cohen

The origin of language as seen by eighteenth-century philosophy
Sylvain Auroux

Cognitive and social aspects of language origins
Alan Barnard

Part 2. At the roots of language

Reconstructed fossil vocal tracts and the production of speech: Phylogenetic and ontogenetic considerations
Jean-Louis Boë, Jean Granat, Jean-Louis Heim, Pierre Badin, Guillaume Barbier, Guillaume Captier, Antoine Serrurier, Pascal Perrier, Nicolas Kielwasser and Jean-Luc Schwartz

Paleoanthropology and language
Ian Tattersall

Material culture and language
Benoît Dubreuil and Christopher S. Henshilwood

Gestural theory of the origins of language
Michael C. Corballis

Part 3. Communication and language origins

Primate communication
Klaus Zuberbühler

FoxP2 and vocalization
Stephanie White

Brain lateralization and the emergence of language
Nathalie Tzourio-Mazoyer and Cyril Courtin

Sensorimotor constraints and the organization of sound patterns
Lucie Ménard

Symbol grounding and the origin of language: From show to tell
Alexandre Blondin Massé, Stevan Harnad, Olivier Picard and Bernard St-Louis

Part 4. Linguistic views on language origins

Sound patterns and conceptual content of the first words
Peter F. MacNeilage

Brave new words
Pierre J. Bancel and Alain Matthey de l'Etang

On the origin of Grammar
Bernd Heine, Gunther Kaltenböck and Tania Kuteva

Arbitrary signs and the emergence of language
Denis Bouchard

On the relevance of pidgins and creoles in the debate on the origins of language
Claire Lefebvre

Part 5. Computational modeling of language origins

Modeling cultural evolution: Language acquisition as multiple-cue integration
Morten H. Christiansen

How language emerges in situated embodied interactions
Luc Steels

Emergence of communication and language in evolving robots
Stefano Nolfi

Evolving a bridge from praxis to language
Michael A. Arbib

Allan R. Bomhard: *A Comprehensive Introduction to Nostratic Comparative Linguistics: With Special Reference to Indo-European.*

Open-access publication. 4 volumes, 2,258 pages, combined into a single PDF. February 2014.

> Allan R. Bomhard has been investigating the possible relationship of Proto-Indo-European to other languages/language families within the context of the Nostratic Hypothesis since the mid-1970s. This book represents his latest contribution to the subject. Spanning four volumes and 2,258 pages, it incorporates, corrects, and expands upon his previous work. The work is divided into three sections, which cover all aspects of the subject in great detail: (1) comparative phonology (including homelands); (2) comparative morphology; and (3) comparative vocabulary. Though the work focuses on distant linguistic relationship, embedded in it is also a fairly complete discussion of comparative Indo-European phonology and morphology. The book was published as an Open-Access work in February 2014 under a Creative Commons License and is available for free download from academia.edu, scribd.com, and Internet Archive.

E.J. Michael Witzel: *The Origins of the World's Mythologies.*

Oxford University Press (New York) | Dec. 2012.

> This remarkable book is the most ambitious work on mythology since that of the renowned Mircea Eliade, who all but single-handedly invented the modern study of myth and religion. Focusing on the oldest available texts, buttressed by data from archeology, comparative linguistics and human population genetics, Michael Witzel reconstructs a single original African source for our collective myths, dating back some 100,000 years. Identifying features shared by this "Out of Africa" mythology and its northern Eurasian offshoots, Witzel suggests that these common myths — recounted by the communities of the "African Eve" — are the earliest evidence of ancient spirituality. Moreover these common features, Witzel shows, survive today in all major religions. Witzel's book is an intellectual hand grenade that will doubtless generate considerable excitement — and consternation — in the scholarly community. Indeed, everyone interested in mythology will want to grapple with Witzel's extraordinary hypothesis about the spirituality of our common ancestors, and to understand what it tells us about our modern cultures and the way they are linked at the deepest level.

Features

- Demonstrates the prehistoric origins of most of the Eurasian and Laurasian mythologies.
- Establishes a basis for much of our ancestral spirituality.

688 pages | 15 maps | 11 b&w halftones | 6-1/8" x 9-1/4" | hardback & paperback

Review

"Not since Frazer's *Golden Bough,* not since Casaubon's *Key to All Mythologies,* has anyone achieved such a grand synthesis of world mythology. Boldly swimming upstream against the present scholarly emphasis on difference and context, Witzel assembles massive evidence for a single, prehistoric, Ur-mythology. An astonishing book."

Wendy Doniger, Mircea Eliade Distinguished
Service Professor of the History of Religions
at the University of Chicago and author of
The Woman Who Pretended to Be Who She Was

MOTHER TONGUE Issue XVIII, 2013

Journal of the Association for the Study of Language in Prehistory (ASLIP)

50th Anniversary of J.H. Greenberg's *The Languages of Africa* (1963)

For subscription and membership information contact:

Secretary-Treasurer: Michael T. Lewis
20 Duane Avenue
West Newton, MA 02465
U.S.A.
lewismtc@rcn.com
Tel. 617-964-0978

ASLIP **membership: $35.00 USD,** yearly
Lifetime membership: $500.00 USD
For electronic payment see ***PayPal*** link at http://aslip.org/

For other information about ASLIP, or to inform ASLIP of important news, new scientific developments, and media relations, please contact:

Information Officer: Jonathan Sherman Morris
São Paulo, Brazil
jonathanmorris1964@gmail.com
Tel: 5511-31512667

For submission of articles, notes, book reviews, or letters to MOTHER TONGUE, please contact:

Editor: John D. Bengtson
Savage, MN U.S.A.
palaeojdb@hotmail.com
Tel. 612-839-3649

For any other correspondence with ASLIP officers and advisors, see the contact information on the inside front cover of this volume.

ISSN 1087-0326 ASLIP Homepage: http://aslip.org